





S. 455.

A  
JOURNAL  
OF  
NATURAL PHILOSOPHY,  
*CHEMISTRY*,  
AND  
THE ARTS.

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VOL. IV.

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Illustrated with Engravings:

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BY WILLIAM NICHOLSON.

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LONDON:

PRINTED BY W. STRATFORD, CROWN-COURT, TEMPLE-BAR FOR  
THE AUTHOR, No. 10, SOHO-SQUARE  
AND SOLD BY  
G. AND J. ROBINSONS, PATERNOSTER-ROW.

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1803.

JOURNAL

NATURAL HISTORY

OF THE



THE

BRITISH MUSEUM

1881

THE BRITISH MUSEUM  
NATURAL HISTORY  
DEPARTMENT  
OF THE  
BRITISH MUSEUM

1881

## PREFACE.

ON the Completion of this fourth Volume I have the Pleasure to give the Names of the Authors and List of Engravings.

The Authors of original Papers are John Gough, Esq.; N. N.; J. Fletcher, Esq.; Ez. Walker, Esq.; J. Harriott, Esq.; Mr. W. Wilson; Dr. Priestley, F. R. S. &c.; Thomas Young, M. D. F. R. S. &c.; Mr. Benj. Hooke; W. N.; John Bostock, M. D.; G. C.; R. B.; The Rev. Wm. Gregor; Mr. Wm. Henry; Z.;  $\pi$ ; and Lawson Huddleston, Esq.—Of foreign Works, G. Dalarive; Rochon; Guiton; Cadet; Gall; Bojames; Klaproth; Jumilhac; Saint Victor; J. B. Berard; L. Reynier; Proust; and Fourcroy.—And of English Memoirs abridged or extracted, R. Chenevix, Esq. F. R. S.; H. Davy, Esq.; S. Tennant, Esq. F. R. S.; Mr. J. Dalton; W. H. Wollaston, M. D. F. R. S.; W. Herschel, L. L. D. F. R. S.; R. Kirwan, L. L. D. F. R. S.; and M. des Lozieres.

Of the Engravings the Subjects are, 1. A new sliding Stop for Air-Pumps and other Uses. 2. A simple and secure Metallic Joint for Tubes. 3. Mr. Harriott's Syphon Engine for raising and lowering Weights. 4. New Method of measuring the Action of Bodies in refracting Light, whether they be transparent or opake; by Dr. Wollaston. 5. Mr. Hooke's Blow Pipe by Alcohol. 6. The Harmonic Sliders of Dr. Young. 7. A strong and flexible Joint for Tubes. 8. Dr.



## PREFACE.

8. Dr. Wollaston's Figures to explain the oblique Refraction of Iceland Crystal. 9. Mr. Dalton's Sketches of the Rivers of England and Wales, and the Districts which supply them with Water. 10. Figure to illustrate Mr. Gough's Theory of sonorous Undulation. 11. The Craniognomic System of Dr. Gall. 12. An Instrument for extracting hard Bodies out of the Œsophagus. 13. A new Reflecting Quadrant by Mr. Ez. Walker. 14. Mr. Huddlestons's Method of floating Boats from one Level to another, without Loss of Water. 15. Engine for blowing up the Stumps of Trees, by Cit. Saint Victor; and 16. Two Drawings of a Magazine Pistol, which discharges Nine Balls in Succession, by once loading.

*Soho Square, April 28, 1803.*

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JANUARY, 1803.

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ARTICLE I.

*On the Nature of the Grave Harmonics. In a Letter from  
Mr. JOHN GOUGH.*

To Mr. NICHOLSON.

SIR,

OUR sense of time arises, as Mr. Locke justly observes, from the constant succession of ideas in the mind ; or to speak perhaps in more intelligible language, it is the result of the attention being occupied by an uninterrupted train of changeable perceptions. Time is on this account, capable of increase and diminution ; it is therefore a species of the abstract term, Magnitude ; in consequence of which, the parts of it possess all the properties contained in the abstract term, Ratio, and are proper objects of the doctrine of proportion. A great variety of proportion takes place amongst the constituent parts of compound beings of the same denomination, and gives birth to a class of phenomena in the philosophy of the human understanding ; which can only be explained by an hypothesis ascribing to man a faculty that compares these ratios, and perceives the effect produced by them. Of this description are the differences perceived by the sense of feeling in the texture of bodies.

Time is a species of magnitude ; and its parts have the habitude of ratio to each other.

The mind compares ratios together as well as their parts. Physical effects thus discriminated.



dies, the discrimination of persons, which is the office of the eye, and those modifications of time; called symphony and harmony, which are judged of by the ear.

The ear affords the conception of a permanent sound, only when the vibrations are too frequent to be separately considered;

Each sense reckons time, by the train of its own ideas alone; the auditory organ on this account measures the lapse of it by sound. Now if the ear had the power of considering the smallest intervals of time apart, it would perceive the constituent vibrations of sounds as separate distinct things, attracting its notice successively; in which case, the human mind could not possibly form a conception of a permanent sound: but it is a maxim in harmonics, that a certain number of vibrations in a second is required to give a sensible degree of duration to the note of a string; I shall at present make use of 12 for this number. The preceding fact ascertains an essential point of my theory, for it fixes the least interval of time that the ear can

but the ratios of frequency are perceived, and excite ideas of acute and grave.

contemplate apart; this organ therefore takes in the gross all magnitudes of this description, which are too small to be examined in detail: in this case it acquires the ideas of acute and grave, by comparing two or more trains of vibrations; the intervals of which are equal in each series taken separately, but greater and less in the whole number taken together.

Two continuous sounds, heard together, have their vibrations arranged in cycles of equal duration perceptible by the ear: If the duration be long enough, each cycle is distinguished;

When two sounds, thus constituted, are heard in concert, the vibrations producing them are arranged in cycles, no one of which continues for a longer or shorter time than the rest; and their effect is perceived by the ear, which becomes sensible of their presence. For when each cycle of a series, separately considered, exceeds the twelfth part of a second, the sense of hearing recognizes each point of division made by the coincidence of the vibrations which separate the contiguous cycles: this circumstance enables the sense to contemplate

if not, the points of recurrence will have the effect of simple periods,

these periods apart, and to comprehend their origin. On the contrary, when the duration of a cycle belonging to a compound series does not exceed the twelfth part of a second, the interval proves too small to be measured by the ear; it therefore escapes notice in a separate state; for the points of division recur too frequently to be observed. When the auditory organ finds itself in circumstances answering to the preceding description, it has but one method to pursue; which is to treat these derivative intervals in the same manner it treats all pe-

and afford a musical sound, or grave harmonic,

riods which are singly too small for its comprehension: it therefore reduces them to a simple musical sound, corresponding

ing in pitch to a string; which vibrates once in the time of each successive cycle: A grave harmonic is on this account always a lower note than any of its constituents, seeing the time of a cycle exceeds the greatest vibration that enters into the composition of it. The strength of a grave harmonic is also weak, when compared with the notes composing it, because these secondary sounds, being nothing more than certain unavoidable efforts of the imagination, they assume the character of a feeble sound, which is just strong enough to be heard in the company of one or more louder tones; for the power of the imagination is always inferior to external impressions, except in fits of insanity, when the organs of sense appear to be blunted by physical causes. This inferiority of the fancy may be assigned as the reason of imaginary sounds, which it frequently creates, being constantly faint and apparently distant.

Dr. Young mentions a trait of the grave harmonics, which spares my own authority, and gives an opportunity to quote his for the following fact, which is of the first moment to the present theory: The grave harmonics always keep the direction of the ears, let the position of the head be changed as oft as you please, resembling in this circumstance a shrill piping note, called the ringing of the ears; which every one ascribes to a slight affection of the auditory duct, because it differs from external sounds in having no fixed direction. The grave harmonics agree with the ringing of the ears in this remarkable particular; which is a strong proof that their immediate cause is seated in the person of the hearer; and it is evident from the nature of things, that this cause originates in the mind, seeing that the organ does not labour at the time under a physical impediment.

I have now given a general theory of these ideal sounds, in compliance with Dr. Young's injunctions, but am not able to perceive any connection between the subject and the question in debate. This was the reason why I disregarded in my last, one of the Doctor's remarks, which he has since called a challenge, perhaps in a petulant manner: a little more argument and a little less ill humour would certainly promote both his cause and reputation. The Doctor endeavours to persuade me, that I misunderstand his idea of coalescence. My idea of the subject is simply this, that sounds cannot coalesce; consequently that all compounds are nothing but a number of distinct

*In proof of which, the fact that it has no direction is urged.*

*Concluding remarks.*



ting cotemporary sounds: if the Doctor will condescend to say, he contends for nothing more, the dispute is at an end; but should he reject this proposa, I advise him to attempt the refutation of my paper by sober arguments, and to transmit his thoughts to that Society which published mine; where, without doubt, they will meet with a candid reception.

JOHN GOUGH.

*Middleshaw, near Kendal,*

*December 13, 1802.*

## II.

*Bagatelles relating to the Pneumatic Apparatus. Sliding Stop-cock; simple and secure metallic Joint for Tubes; Improvement of Read's Apparatus; original Invention of Woulfe's Bottles and Tubes; and of a Method of closing Vessels. By N. N.*

Invention of  
sliding stop-  
cocks.

§ 1. THE present bagatelle writer has been frequently at a loss, in his pneumatic experiments, to meet with stop-cocks both cheap and perfectly air-tight, or to have them easily repaired when out of order. In a conversation he had on this subject with Dr. Fischer, *ci-devant* Astronomer at Manheim, he understood that Mr. Helfensrieder, while Professor of Natural Philosophy at Ingoldstadt, to remedy similar defects in his air-pumps, used steel parallelopipedons with a vertical and horizontal opening joined at right angles, and sliding between brass-work. This furnished the writer with the idea of constructing sliding stop-cocks for a gazometer; he was constructing similar instruments. He lays no claim to the invention; but has found them answer extremely well.

Description of  
the cock.

Fig. 1. Pl. I. exhibits a section of the sliding stop-cock put together.

Fig. 2. One of the brass parallelopipedons (*a a*, Fig. 1) against which the pieces *b* and *c*, Fig. 1, are pressed and united by means of four screws.

Fig. 3. The slider by which, when brought home on one side, the stop-cock is opened, and, on the contrary, shut when moved flush on the opposite extremity.

N. B. The bore of the whole is three-eighths of an inch diameter throughout, which is of the greatest advantage, when any

any gas is immediately transferred from the disengaging vessel to the gazometer; for if the apparatus be sufficiently air-tight as it ought to be, and the gas rapidly disengaged, an explosion may be occasioned for want of a sufficient passage for the gas.

Fig. 4. The piece *c*, Fig. 1. Its companion *b* ends in a male screw to be united to the gazometer.

Fig. 5. A perspective view of the stop-cock, the above enumerated parts being put together.

Fig. 6. A nut provided with a female screw soldered to the gazometer, by means of which the stop-cock may be applied or removed as usual.

Fig. 7. represents two brass cylinders. One end *f* is fitted by grinding to the orifice of the stop-cock; and to the other ends *g* and *h*, between *x*, is fastened a flexible tube for experiments with the blow-pipe.

This flexible tube was made by twisting a brass-wire spirally round a long thin cylinder, covering this with oiled silk twice wrapped round, and fastened by means of thread between the grooves of the wire. It was then again varnished and covered in a spiral manner with sheep-gut slit longitudinally, and again secured with thread. Lastly, to protect the whole from external injury, it was covered with leather in the same manner as the tubes of the inhalers. These flexible tubes answer the same purpose as the very costly ones made of elastic gum, similar to the hollow bougies made for surgeons.

Cheap and very useful flexible tube for the gases.

Fig. 8. A (broken) blowpipe, put in this place merely to shew how its end *k* is connected with the gazometer, by fitting into the end (*f*) of the brass end of the flexible tube.

§ 2. In pneumatic experiments for transferring any gas, for instance oxygen, from the air-holder to another vessel, it is frequently necessary to have tubes which can be joined in certain directions and inclinations; and for this purpose a contrivance has been adopted, which is well known, and is represented Fig. 1. Plate II. where *a* and *b* are spherical segments, and *c* their junction by means of a screw. But as this apparatus, besides that its parts are with difficulty well ground together, is liable to become imperfect, more especially by strong pressure, or a blow received on the edges of the segments, or a grain of dusty sand interposed between the small flat surfaces, the writer of these trifles has contrived another, A better delineated

Description of a joint for metallic tubes.

A better construction.

delineated Fig. 2. A B C are the parts corresponding to those of the preceding. It serves as a connecting piece, to which tubes at D and H may be joined in the requisite directions. Its construction and advantages are sufficiently obvious from the mere inspection of the drawing, so that nothing is requisite to be added on this account. Several friends had made it after this model, and highly recommend its use: and since the making of it the writer understands that the ingenious Mr. Webster, whose merits were never properly estimated by the great improver of culinary utensils, had the same thought, and executed it about two years ago; he, therefore, does not claim the merit of its first invention, but only (perhaps) that of publishing it.

The sliding stop-cocks, § 1, as well as these connecting pieces, § 2, have been executed in a masterly manner by Mr. Hooke, optician and mathematical instrument maker, No. 159, Fleet-street.

Various improvements in Read's funnel apparatus.

§ 3. In Mr. Read's very simple and cheap pneumatic apparatus (see Nicholson's Journal, new series, Vol. III. page 55) two improvements have since been made. The first, that the top of the exterior tube is not soldered to the funnel I (Plate IV. *cit.*) but has a top screwed on with a collar of leathers, and the upper part of the innermost tube A F projects above it  $1\frac{1}{2}$  inch, and receives the brass cylindrical end of the funnel, fitted to it by grinding. The second improvement has been made by Mr. Hooke, who instead of soldering all the parts together, has joined the interior parts to the outer tube A A, in the middle about E. By these means, if any impurities that might impede the action of the instrument, should happen to settle at the bottom C or D, or at L, the whole may be easily taken asunder and cleaned.

Woulfe's apparatus was invented by Glauber.

§ 4. The whole chemical world speaks, hears, and writes of Woulfe's pneumatic apparatus. The tribute of merit must not be denied to Mr. Woulfe; but perhaps it may be curious to know, that the original invention of this contrivance, in the strict sense, belongs to good honest father John Rudolph Glauber. If any gentleman doubt of this fact, let him consult Glauber's works, translated into English by Christopher Packe, and printed, Lond. 1689, in folio, for the translator, by Thomas Milbourn. The very first plate to Glauber's Treatise on Philosophical Furnaces shews it, Fig. 3.

§ 5. An-



§ 5. Another bagatelle of this kind, relating to pneumatic instruments, is a pretended new method of closing bottles and other vessels air-tight *without luting or grinding*, which, a few years past, I saw recommended (I do not recollect in which of our Journals), and which consists in having a groove round the neck into which a cap fits, see Fig. 3; so that this groove (*a*) may be charged with water or mercury as circumstances require. This likewise belongs originally to Glauber, as may be seen in the plate just before quoted of the translation of his works.

There is *nothing new under the sun*, says Solomon, *Eccles. I. v. 10.* and more of this kind of *old news* shall be occasionally given by,

SIR,

Your humble servant,

N. N.

December 12, 1802.

### III.

*Analysis of Corundum, and of some of the Substances which accompany it; with Observations on the Affinities which the Earths have been supposed to have for each other in the humid Way.*  
By RICHARD CHENEVIX, Esq. F. R. S. and M. R. I. A.  
*From the Philosophical Transactions for 1802.*

#### § I.

SOME kinds of corundum, such as the adamantine spar of China, and the sapphire, have already been analyzed by Mr. Klaproth. This would have rendered any further experiments unnecessary, were it not, that I have had at my disposal many kinds of corundum he did not possess, and also some substances accompanying it, which were unknown before the preceding communication of the Count de Bournon.

As, from the result of my analyses, it appears that all the different kinds of corundum are nearly similar in their constituent parts, and differ only in their proportions, it would be tedious to mention every experiment I made upon each kind. I shall therefore confine myself to stating, once for all, such modes

All the kinds are similarly constituted.

Kinds of corundum not examined by Klaproth.

modes of analysis as were employed with stones of a similar nature; and then present a summary of the results: lastly, I shall conclude with an enquiry into a much contested point, which lately threatened a revolution in docimaistic chemistry.

Principal character: Extreme hardness.

A principal character of corundum in general, as may be found in the Count de Bournon's mineralogical description, is extreme hardness; and thence the difficulty of reducing that substance into fine powder will be easily conceived. We are told by docimaistic chemists, that the most advantageous method of pulverizing hard stones, is to make them red-hot; and, in that state, to plunge them into cold water. But I found that this operation, when performed but once, was by no means sufficient for corundum. I therefore repeated it, till the stone appeared to be fissured in every direction. After this, the specimen to be pulverized was put into a steel mortar, about three-fourths of an inch in diameter, and three inches in depth, into which a steel pestle was very closely adjusted. A few blows upon the pestle caused the stone to crumble; and the fragments were then easily reduced into an impalpable powder, in an agate mortar, with a pestle of the same material. The abrasion from the mortar, usual in the pulverization of hard stones, was much diminished by the above precaution; rubies and sapphires being, in a short time, ground to a powder nearly as minute as the finest precipitate.

Pulverized after repeated ignition and quenching.

Efficacy of the process.

The great difficulty of fusing corundum with alkali, renders that process objectionable.

M. Klaproth in his analysis before-mentioned, had observed with how much difficulty the stones were acted upon by potash or soda. I found that the greatest heat a silver crucible could support, without melting, was not sufficient to produce a satisfactory fusion of one part of corundum, with six parts of either of those alkalis; nor did an exposure to that temperature during several hours, seem to render the treatment more effectual. Not more than half the quantity of the corundum was ever rendered soluble in any acid; and what remained was the powder of the stone, wholly unchanged. The repeated filtrations and evaporations with which this treatment must be attended, not only render it tedious, but also produce uncertainty in the results. Even when very finely powdered corundum was exposed, with six times its weight of potash, in a platina crucible, to a heat of  $140^{\circ}$  of Wedgwood, for two hours together, it was not acted upon in such a manner as to

be

be fit for analysis. From all these experiments I concluded, that some more efficacious mode of rendering corundum soluble in acids was to be sought.

I boiled a great quantity of sulphuric acid upon very finely powdered corundum in a platina crucible. But, although the acid, after a great length of time, had dissolved a little of the stone, I did not find this method more satisfactory than the others. Nitric, muriatic, and nitro-muriatic acids, were less effectual than the sulphuric. Phosphoric acid, held in fusion with corundum, did not dissolve any notable portion of that stone, or render it soluble in other acids.

I then had recourse to sub-borate of soda (borax), which I found to answer beyond my expectation. Two parts of that salt, calcined, and one of corundum, enter into fusion, at a temperature which I judged to be about 80° of Wedgwood \*, and a glass, more or less coloured, is formed. This glass is soluble in muriatic acid; and, by this method, it is easy to obtain a complete solution of corundum. My general method of operating was as follows:

I took one hundred grains of corundum; and, having several times made it red-hot, and plunged it into cold water, I put it into the steel mortar, and treated it as already mentioned. I then poured some very dilute muriatic acid upon it, to wash off whatever iron might have adhered, in consequence of its mechanical action upon the mortar. After it was dried and weighed, I put it into the agate mortar, and ground it as fine as I could. The augmentation of weight was then noted; and was always taken into account in the general result. I then put the whole into a platina crucible, with 200 grains of calcined sub-borate of soda, and exposed the mixture for an hour or two to a violent heat. When the crucible was cool, muriatic acid was boiled upon it and its contents; and, in about twelve hours, all the glass disappeared. If I wished to obtain the silica directly, I evaporated the whole to dryness; but, if otherwise, I precipitated by an alkaline carbonate, and washed the precipitate, in order to get rid of all the salts contained in the liquor. This latter mode I believe to be preferable. I then re-dissolved the precipitate in muriatic acid, and evaporated for silica. But, as corundum contains only a small portion of this earth, there

Neither the volatile acids nor the phosphoric have any notable action on corundum.

Borax fuses it well. Two parts of calc. borax and one of cor. form a glass soluble in muriatic acid.

Analysis. 100 corundum were pulverized; fused with 200 borax; dissolved in muriatic acid; (evap. to dryness for silica, or) precip. by alk. carbonate; abstract the salts by water; redissolve the precip. in mur. acid; evap. for silica; filter the mur. solution; precip. by heat with excess of potash; oxide of iron is permanently deposited, but alumina taken up by the alk.; precip. the alumina by mur. amm. Wash, dry, ignite, and weigh the earths.

\* I have no doubt that a lower temperature would be sufficient.



was little or no appearance of jelly. When the filica was thus precipitated by evaporation, I filtered the liquor, and boiled it with an excess of potash. By this operation, the alumina was precipitated, and then re-dissolved by the excess of potash, from which it was finally obtained by muriate of ammonia; the iron which had remained undissolved by the potash, having of course been previously separated from the alumina. This earth, and the filica, after being washed and dried, were ignited, and thus the weight of both was obtained.

Example. Analysis of the sapphire.

I shall exemplify, in a single instance, this mode of treatment; and then present the results obtained from the different kinds of corundum. For this purpose, I shall select the blue perfect corundum, or sapphire, as the stone which has been the most ably analyzed by Mr. Klaproth. From a view of both analyses, the efficacy of the fusion with borax will be evident; and the results of the several experiments may be compared.

100 grs. sapphire were pulverized and then fused into a green glass with 250 borax,

1. 100 grains of sapphire, pulverized in the agate mortar, as above stated, had increased to 105. These 105 were mixed with 250 of calcined sub-borate of soda, and put into a platina crucible. They were then exposed to a violent heat for two hours, and afterwards allowed to cool. The mass was vitrified, and had the appearance of a greenish blue glass, fissured in many directions.

which was dissolved in boiling mur. acid.

2. This glass being strongly attached to the platina crucible, the whole was put into muriatic acid, and boiled for some hours. By these means, a total and limpid solution was obtained.

Precip. by carb. of ammonia, was washed, dried, dissolved in mur. and expos. to evap. it gave a precip. of 10.25 filica.

3. The matter of the stone was next precipitated, by ammonia not entirely saturated with carbonic acid; the liquor was filtered; and the precipitate well washed and dried. It was then redissolved in muriatic acid, and evaporated.

4. By this evaporation a precipitate was formed, which, when well washed and ignited, weighed 10,25 grains, and was filica.

The water of washing and the mur. liquor were boiled with excess of potash. One grain of iron remained. Mur. of amm. was added to the alk. solution.

5. The liquor, together with that which had washed the precipitate, was boiled in a silver vessel, with an excess of potash; this redissolved all the precipitate, except one grain.

6. Muriate of ammonia was poured into the alkaline solution. (No. 5.) The potash expelled the ammonia from the muriatic acid, and, forming muriate of potash, could no longer retain the earth in solution; a very copious precipitate, therefore, was formed.

formed. This precipitate had all the properties of alumina; The potash left and, when well washed and ignited, weighed 92 grains. Con- united with the frequently, deducting 5 from the filica, for the abrasion of the mur. acid, and suffered the alumina to fall. mortar, we shall have for result, Washed and ignited it weighed 92 grs.

Silica	-	-	-	-	5,25
Alumina	-	-	-	-	92
Iron	-	-	-	-	1
Loss	-	-	-	-	1,75

Comp. parts deduced.

100,00.

The chief difference between these proportions and those established by Mr. Klaproth, is in the filica. That chemist found little filica in his analysis; did not find any notable portion of it in the specimens he examined. This naturally induced me to make a very strict re- this point was search into every possible means by which any filica might have particularly examined been introduced into the results; whether by the borax, the alkali, or any of the other re-agents I had used. But, finding very clearly, that none of these substances did contain any, I could no longer hesitate to believe, that the proportion I have here stated, was actually contained in the sapphire I analyzed. I am likewise convinced, that no more than the quantity I have mentioned was worn from the agate mortar and pestle; for my constant practice was, to weigh them, both before and after I had used them, in scales which, when charged with four pounds on each end, turn easily with the tenth part of a grain.

The general results, from all the different kinds of corundum, were as follows:

<i>Blue perfect Corundum, or Sapphire.</i>						<i>Red perfect Corundum, or Ruby.</i>						Component parts of diff. kinds of corundums.
Silica	-	-	-	-	5,25	Silica	-	-	-	7		
Alumina	-	-	-	-	92	Alumina	-	-	-	90		
Iron	-	-	-	-	1	Iron	-	-	-	1,2		
Loss	-	-	-	-	1,75	Loss	-	-	-	1,8		
<hr/>						<hr/>						
100,00.						100,0.						
<i>Imperfect Corundum from the Carnatic.</i>						<i>Imperfect Corundum from Malabar.</i>						<i>Imperfect</i>
Silica	-	-	-	-	5	Silica	-	-	-	7		
Alumina	-	-	-	-	91	Alumina	-	-	-	86,5		
Iron	-	-	-	-	1,5	Iron	-	-	-	4		
Loss	-	-	-	-	2,5	Loss	-	-	-	2,5		
<hr/>						<hr/>						
100,0.						100,0.						



<i>Imperfect Corundum from China.</i>					<i>Imperfect Corundum from Ara.</i>				
Silica	-	-	-	5,25	Silica	-	-	-	6,5
Alumina	-	-	-	86,50	Alumina	-	-	-	87,0
Iron	-	-	-	6,50	Iron	-	-	-	4,5
Lofs	-	-	-	1,75	Lofs	-	-	-	2,0
<hr/>					<hr/>				
100,00.					100,0.				

Their diff. of colour arises from the oxidizement of the iron.

As I could not discover chrome, or any other colouring substance, except iron, in these stones, I can attribute their difference of colour only to the different state of oxidizement of the iron; but it is impossible to ascertain what that state may be, from so small a quantity.

The matrices of these stones submit to the usual analysis.

The matrices of these stones, and the substances accompanying them, are more easily fused than the six kinds of corundum just mentioned. The usual and well known mode of treatment by potash, was sufficient to render these substances soluble in the acids. Since the many experiments of Klaproth, Vauquelin, and others, the mode of analyzing mineral bodies is become so familiar to chemists, that I shall mention particulars with respect to one only of the following substances.

### MATRIX OF CORUNDUM FROM THE PENINSULA OF INDIA.

Matrix of Indian corundum. 100 grs. fused with potash; dissolved in mur. acid; evaporated; dissol. in sm. excess of acid; left 42,5 of silica.

1. A certain quantity of this matrix was reduced to powder, in the manner already described. 100 grains of it were treated with potash, in a silver crucible: they then afforded a limpid solution in muriatic acid. The liquor was evaporated; and, long before the mass was entirely dry, it had assumed the appearance of a jelly. When the saline matter in the evaporating-dish was dissolved in a slight excess of acid, a white powder remained at bottom, which had all the properties of silica, and, when washed and ignited, weighed 42,5 grains.

Ammonia threw down alumina and oxide.

2. Into the liquor which had served to wash the above powder, I poured ammonia. A copious precipitate was thus formed, which was separated by filtration, and well washed.

Carb. potash threw down carb. lime = lime 15.

3. Carbonate of potash also caused a precipitate in the liquor of No. 2. This precipitate was found to be carbonate of lime, and weighed 23,5 grains, = 15 of lime.

The alumina and oxide were dissol. in mur. acid; and the

4. The precipitate of No. 2. was redissolved in muriatic acid; then boiled with an excess of potash, and filtered. There remained undissolved, 3 grains, which were iron.

5. The

5. The liquor of No. 4. was precipitated by muriate of ammonia, and afforded alumina; which, being washed and ignited, weighed 37,5 grains.

I could also perceive a trace of manganese.

The proportions therefore are,

Silica	-	-	-	-	42,5
Alumina	-	-	-	-	37,5
Lime	-	-	-	-	15,0
Iron	-	-	-	-	3,0
Lofs, with a trace of manganese	-	-	-	-	2,0
					<hr/>
					100,0.

alumina taken  
up by potash.  
3 grs. iron re-  
mained.  
Mur. amm. pre-  
cipitated 37,5  
alumina.  
Manganese, a  
trace.  
Comp. parts.

By a similar treatment, the following substances, contained in this matrix, afforded the under-mentioned results.

<i>Felspar.</i>					Component parts of felspar;
Silica	-	-	-	64	
Alumina	-	-	-	24	
Lime	-	-	-	6,25	
Iron	-	-	-	2,00	
Lofs	-	-	-	3,75	
					<hr/>
					100,00.
<i>Fibrolite.</i>					of fibrolite;
Silica	-	-	-	38	
Alumina	-	-	-	58,25	
A trace of iron, and lofs	-	-	-	3,75	
					<hr/>
					100,00.

This is the only stone I have ever met with, that yielded nothing but silica and alumina; for the quantity of iron was so small as hardly to be taken into account. I have repeated this analysis three times, and have not found a difference of half a grain.

*Thallite in Crystals, with a rough Surface.*

					of rough thallite;
Silica	-	-	-	45	
Alumina	-	-	-	28	
Lime	-	-	-	15	
Iron	-	-	-	11	
Lofs	-	-	-	1	
					<hr/>
					100.

*Thallite*

of prismatic  
thallite ;

*Thallite in Prisms like the Tourmalin.*

Silica	-	-	-	-	40
Alumina	-	-	-	-	25
Lime	-	-	-	-	21,5
Iron	-	-	-	-	11,5
Loss	-	-	-	-	2
					<hr/>
					100,0.

of thallite in  
transparent frag-  
ments ;

*Thallite in Fragments, of a fine transparent Yellow Colour.*

Silica	-	-	-	-	42
Alumina	-	-	-	-	25,5
Lime	-	-	-	-	16
Iron	-	-	-	-	14
Loss	-	-	-	-	2,5
					<hr/>
					100,0.

of fibrolite of the  
Chinese corun-  
dum ;

*Fibrolite accompanying the Matrix of Corundum from China.*

Silica	-	-	-	-	38
Alumina	-	-	-	-	46
Iron	-	-	-	-	13
Loss	-	-	-	-	3
					<hr/>
					100.

of felspar from  
sand of Ceylon.

*Felspar from the Sand of Ceylon.*

Silica	-	-	-	-	68,5
Alumina	-	-	-	-	20,5
Lime	-	-	-	-	7
Iron	-	-	-	-	1,5
Loss	-	-	-	-	2,5
					<hr/>
					100,0.

Metallic cru-  
cibles alone must  
be used with  
earths ;

As the greater part of the above substances were fusible without difficulty in potash, I preferred using a silver crucible to any other. It may be laid down as a general rule, with respect to delicate experiments, that in the treatment of metallic substances, we should not use metallic crucibles ; but, in the treatment of earthy bodies, they alone are to be depended upon. The easily oxidizable metals cannot be employed ; but silver and platina present advantages which no other metals seem

namely, silver  
and platina.



seem to possess. Theory would certainly give a general preference to platina, from its resistance both to heat and to acids; and practice will justify this preference, in all but a single instance. If a quantity of potash be kept for some time in fusion, in a platina crucible, it will be found that the crucible has lost several grains of its weight. The platina so dissolved may be looked for in the potash; and, if this be saturated with muriatic acid, and evaporated, we shall find the well-known triple salt, formed by the combination of muriatic acid with potash and oxide of platina. This action of potash upon platina, does not depend upon any mechanical cause, such as friction, the force that determines it being purely chemical. If a salt formed by potash, or a salt formed by ammonia, be mixed with a salt of platina, a precipitate ensues, which is a triple salt; and it is by this method, that the Spanish government detects the platina, in the ingots of gold sent from their American possessions. It is therefore evident, that an affinity does exist between potash and platina, in a certain state; and I imagine it to be this affinity, which causes the oxidizement of the platina, when potash is kept in fusion upon that metal. I must however observe, that my crucible was prepared by Janetty, in Paris, according to a method he has published in the *Annales de Chimie*: and that he always employs arsenic, a little of which certainly remains united to the platina. What influence arsenic may have, remains to be determined. Soda does not form a triple salt with the oxide of platina; for I have frequently kept this alkali in fusion, in a platina crucible, for a long time; yet very little action was produced upon the metal. This fact seems to corroborate my assertion, that the affinity of potash for oxide of platina, determines the oxidizement of the metal.

Platina is preferable; except with potash which acts upon it.

Spanish method of detecting platina in ingots of gold. The salts of platina are precipitated (triple) by the salts of potash.

Is platina affected by the arsenic it contains?

Soda scarcely acts upon platina.

Whenever I suspected that platina had been dissolved, I could easily detect the smallest portion of it. A solution of platina, so dilute as to be nearly colourless, manifests, in a very short time, the colour of a much more concentrate solution, and becomes reddish, by the addition of a solution of tin in muriatic acid. This I have found to be, by many degrees, the most sensible test for platina; and it would answer the purposes of the Spanish government, much better than that they usually employ.

Muriate of tin is the most delicate test of platina.

Alkalis do not act on silver; yet the crucibles become rather more brittle by long use.

The fixed alkalis rise by mere heat; potash more readily than soda.

Water assists their elevation.

Potash usefully volatilized in bleaching.

The alkalis have no immediate action upon silver; but I have observed, that crucibles of this metal, after they have been a long time in use, become somewhat more brittle than they were before.

Potash and soda have long been termed fixed alkalis; and it is certain that, if we compare them with ammonia, they are so. But *fixed* is an absolute term, and cannot admit of degrees. If potash, such as we obtain from Mr. Berthollet's method of preparing it, be kept in fusion at a very strong heat, it may be totally volatilized. The vapour of the alkali may be perceived in the room; and vegetable colours will undergo the change which is usually produced by alkalis. Indeed, in preparing Mr. Berthollet's potash, the vapour of the alkali may be easily perceived. Soda is not quite so volatile; though far from being fixed. It appears also, that a little water increases the volatility of both potash and soda, as happens with boracic acid. This volatility of potash, has been advantageously applied of late to the art of bleaching.

## § II.

*On the Affinities the Earths have been supposed to have for each other, in the humid way.*

In the course of the foregoing analysis, I had occasion to make some further observations concerning a subject upon which I had been formerly engaged, namely, on the affinities the earths have been supposed to have for each other, when held in solution by acid or alkaline menstrua.

On the affinities of the earths for each other.

Historical facts.

Guyton's experiments.

In the XXVIIIth volume of the *Annales de Chimie*, page 189, I published a paper upon the analysis of some magnesian stones. In this paper, I took notice of the following affinities of the earths for each other, namely, the affinity of alumina for magnesia, of alumina for lime, and of alumina for silica. In the XXXIst volume, page 246, there is a memoir, by Guyton de Morveau, upon a similar subject;\* and he there reports some experiments of his own, by which he was induced to think, that the earths do really possess a chemical attraction for one another. Since that time, the affinity of the earths has been

\* He has taken no notice of any of the experiments contained in my paper.

received



received among chemists as an undoubted fact; and, at the end of Mr. Kirwan's *Essay on the Analysis of mineral Waters*, we find a list of earthy salts which produce a re-action upon one another, supposed to be caused by an affinity that tends to unite their bases, in the form of a precipitate, insoluble in the acids. Some other detached observations are to be found, in the *Journal de Physique*, and in the *Annales de Chimie*. The fact is certainly one of the most important in the doctimastic art, and merits all the attention of the skilful in that branch.

In the XLth volume of the *Annales de Chimie*, page 52, <sup>contested by Darracq.</sup> Darracq has published a paper, intended as a refutation of the conclusions drawn by Guyton. I had myself repeated the greater part of the experiments of the latter; and the results I obtained were exactly similar to those of Darracq. In fact, I had intended to continue the researches; but the very satisfactory paper of Darracq, appeared to me to render a further prosecution of them totally useless. However, a paragraph inserted in the *Annales de Chimie*, (Tom. XLI. p. 206.) and of which Guyton appears to be the author, shews that he has not derived from the Memoir of Darracq, that conviction which it certainly conveys. The paragraph in question is founded upon a letter, written from Freyberg, by Dr. G. M. to Dr. Babington, dated December 17, 1800, and inserted in the IVth volume of Nicholson's *Journal*, page 511. This letter contains an opinion which deserves to be canvassed, as it is not perfectly just; and the use Guyton has made of it, has determined me to add my observations to those of Darracq.

I shall follow the order of Guyton's experiments, in the <sup>Repetition of</sup> enumeration of those I made. <sup>Guyton's experiments.</sup>

*Exp. 1.* From a mixture of lime-water and barytes-water, <sup>Lime-water and barytes-water</sup> Guyton obtained a precipitate. I obtained none. <sup>= precipitate.</sup>

*Exp. 2.* A solution of alumina in potash, mixed with a solution of silica in the same, gave a precipitate, after standing some time. This had been observed by Darracq, and by Guyton, and agrees perfectly with the affinity which, before Guyton published his paper, I had asserted to exist between these two earths. <sup>Failed.</sup> <sup>Alumina in potash and silica in potash = precip.</sup> <sup>—Ascertained before.</sup>

*Exp. 3, 4, 5.* Lime-water, strontia-water, and barytes-water, produce a somewhat similar effect upon a solution of silica in potash. <sup>Water of lime, or of strontia, or of barytes + to silica in potash = precip.</sup>

Barytes water,  
and strontia-  
water = no pre-  
cipitate.

Mur. lime and  
mur. alumina  
= precip.—  
Failed.

Mur. lime and  
mur. magnesia,  
= no precip.

Mur. lime and  
mur. barytes =  
precip.—Failed.

Mur. mag. and  
mur. alum. =  
no precip.

Mur. magnesia  
and mur. of ba-  
rytes or strontia  
= abundant  
precip.—Failed.

Mur. alum. and  
mur. barytes =  
precip.—Failed.

Mur. barytes  
and strontia =  
no precip.

Mur. strontia  
and alumina =  
no precip.

Guyton was  
wrong in assert-  
ing that barytes  
has affinity for  
lime, magnesia  
and alumina;  
and that strontia  
attracts alumina.

*Exp. 6.* No precipitate took place from a mixture of barytes-water and strontia-water; nor from solutions of the carbonates of those earths, in water impregnated with carbonic acid.

*Exp. 7.* Guyton obtained a precipitate, by mixing solutions of muriate of lime and muriate of alumina. I could not obtain any.

*Exp. 8.* Solutions of muriate of lime and muriate of magnesia, when mixed, did not afford a precipitate.

*Exp. 9.* Muriate of barytes did not, as Guyton has asserted, form a precipitate with muriate of lime. He was right in saying, that muriate of strontia gave no precipitate with muriate of lime.

*Exp. 10.* Muriate of magnesia and of alumina, afforded me no precipitate. Guyton says, that the liquors became milky.

*Exp. 11.* Muriate of magnesia, whether mixed with muriate of barytes or of strontia, afforded me no change; although Guyton says he obtained an abundant precipitate, by mixing muriate of magnesia with muriate of barytes.

*Exp. 12.* Muriate of alumina and of barytes, did not, when mixed together, yield any precipitate. Guyton asserts, that there is a precipitate in this case.

*Exp. 13.* Muriate of barytes and of strontia, did not form a precipitate. Guyton has remarked the same.

*Exp. 14.* From muriate of strontia and of alumina, I obtained no precipitate. With Guyton the liquor became milky.

From all these experiments it appears very clearly, that Guyton has pronounced too hastily, upon the affinity which he supposes barytes to entertain for lime, for magnesia, and for alumina; and that he is equally in the wrong, with regard to the affinity of strontia and alumina. With regard to *Exp. 3, 4, and 5*, although they appear to be true, yet it would require the respective precipitates to be further examined, before we admit a decided affinity between the earths. The quantity of carbonic acid also, which must of course combine with the potash, during the treatment of the silica by that alkali, should be taken into account, in considering the cause of the precipitate.

The



The solutions which I used, of all the above salts, were in the most concentrate state; therefore, in the state most favourable for showing precipitation, if any had taken place.

It is not very difficult to account for the appearances that deceived Mr. Guyton in his experiments, and for the cause that produced them. In one instance, he obtained a precipitate from muriate of lime and of alumina, because, in all probability, the alumina he dissolved in muriatic acid had been precipitated from alum; and alumina, thus prepared, retains a small portion of sulphuric acid \*. In the next place, it is very likely that his solutions were sufficiently concentrate to give a precipitate of sulphate of lime. The same was the case with regard to his mixture of muriate of strontia with muriate of alumina. As to the general conclusion, that barytes has an affinity for lime, magnesia, and alumina, which strontia does not appear to possess, it is to be explained as follows. Lime often contains a little sulphate of lime. Mr. Guyton's magnesia, as well as his alumina, had probably been obtained from the sulphate; and we are indebted to Mr. Berthollet, for the true nature of many similar precipitates.

Causes of the errors in his experiments

Barytes is a much more delicate test than strontia, for sulphuric acid; and therefore, barytic solutions were affected by quantities of sulphuric acid, which strontia could not render sensible. This I have ascertained to be the case: for I have obtained copious precipitates, by barytes, in a liquor composed for the purpose, wherein strontia did not produce the smallest cloud, or show the presence of sulphuric acid.

A little care and attention are necessary, in preparing the earths, which are to be dissolved in the muriatic acid, for these experiments; and, if Mr. Guyton had taken the requisite precautions, he would not have been led into error. The object to be kept in view is, to free the earth from sulphuric acid; and, if this be obtained, there is not the smallest precipitate or cloud, in any of the cases I have mentioned. If

He did not take due care in preparing his earths.

\* It is somewhat singular, that Guyton should have observed this fact elsewhere. See his experiments on the diamond, in the *Annales de Chimie*. The preparation of a barytic salt, by alumina prepared from the sulphate of this earth, had been observed by Scheele, in his *Essay on the Affinities of Bodies*. But that great chemist referred the phenomenon to its right cause, viz. to some sulphuric acid remaining in all alumina thus prepared.

any further proof be necessary, with regard to the cause of precipitates obtained in the manner stated by Mr. Guyton, I may add, that I have repeated his experiments, and have always found the precipitates to be sulphate of barytes.

Mr. Kirwan has made similar mistakes.

The general conclusion to be drawn from the observations of Mr. Kirwan, already alluded to, is, that barytes has an affinity for lime, magnesia, and alumina, upon which earths *strontia* does not seem to have any influence. But these mistakes are to be accounted for in the same manner as those of Mr. Guyton, viz. by sulphate of barytes being much less soluble than sulphate of *strontia*, and therefore showing the presence of a smaller portion of sulphuric acid, or, in other words, being a much more delicate test for that substance.

Letter in Nicholson's Journal,

With regard to the letter already mentioned as being inserted in Nicholson's Journal, and which drew some reflections from Mr. Guyton, it is necessary to examine as much of it as may be thought objectionable.

asserting that because a precipitate of *silica* is soluble in acids, the results of analysis with alkalis must be fallacious.

The author says, that he repeated the experiments of Mr. Guyton, with an alkaline solution of *silica* and alumina, and that he obtained a precipitate; which precipitate, though containing *silica*, was totally soluble in the acids. "Here," he says, "the properties of the *silica* must be considerably altered. This must render all analysis with alkalis suspicious; and shews on what fallacious grounds the proud dominion of chemistry rests, which she has exercised so long, in such an arbitrary and overbearing manner, in the mineral kingdom." This opinion is by no means likely to overthrow the pretensions of chemistry; for the very circumstance of rendering *silica* soluble in the acids, is one of the discoveries that has most contributed to render certain, and to extend, our knowledge of analysis. No earthy substance is now thought fit to be submitted to further experiment, till a complete solution of it in an acid be first obtained; and, when that solution cannot be effected directly by the acid, it is always attempted by previous fusion with an alkali. This mode of rendering *silica* soluble in acids, is no new discovery; it has been long known; and the analysis of minerals has never been brought so near to truth, as since it has become an indispensable condition.

This fact is on the contrary the greatest improvement in modern analysis.

Alumina attracts *silica*;

I have no doubt as to the fact of a precipitate being formed, by mixing together an alkaline solution of *silica* and alumina. Alumina indeed appears to exercise an attraction, as I before stated,



stated, for silica, for magnesia, and for lime. All stones in which there is but little alumina, and a great quantity of silica, leave, after fusion with potash, a light and flocculent substance, which cannot be dissolved by the acids: this substance, however, which is silica, has been in solution in the alkali. But, if a greater proportion of alumina be present, none of this flocculent precipitate appears; hence it is evident, that alumina must determine its solution. Its easy solubility, in the latter case, cannot depend upon the division of the particles of the silica in the stone; for, in the first place, after being fused with potash, the tenuity of the particles of every stone must be nearly the same; and, in the next place, I have not observed, that any earth, except alumina, can promote the chemical solution of the silica, though they must all occasion its mechanical division.

As to the affinity of alumina for magnesia, it is by much the most powerful of all those which any of the earths have for each other. I attempted to precipitate magnesia from muriatic acid, by ammonia, even in excess; but found that the whole muriate of magnesia had not been decomposed, and that a triple salt, or an ammoniacal muriate of magnesia \*, had been formed. I then poured an excess of ammonia into a solution of muriate of magnesia, mixed with a large proportion of a solution of muriate of alumina. All the earth was precipitated; and nothing remained in solution, except muriate of ammonia. The liquor was then filtered, and the precipitate washed and dried. I dissolved it in muriatic acid, and boiled it with a great excess of potash. Some alumina was taken up, but by no means all the quantity that had been used. The precipitate which had resisted the action of potash, was again dissolved in muriatic acid, and precipitated by carbonate of potash. The carbonate of magnesia was held in solution by the excess of carbonic acid; and, by using potash and carbonic acid alternately, (the first to dissolve alumina, the second to dissolve carbonate of magnesia,) I effected a separation of the earths. These experiments shew, that there is an affinity between alumina and magnesia, and a certain point of saturation, where the action of potash upon alumina is wholly counteracted by the affinity of that earth for magnesia.

and promotes its solubility.

Alumina very powerfully attracts magnesia.

\* This salt is well known in chemistry.

When

Alumina attracts lime; which becomes dissolved in potash along with it.

When a solution of potash is boiled upon a mixture of lime and alumina, the alumina is dissolved, together with a much greater portion of lime than can be attributed to the dissolving power of the water alone. But, if a solution of potash be boiled upon lime, without alumina, no more lime is taken up than would have been dissolved by an equal quantity of water not containing potash in solution; consequently, alumina seems really to promote the solution of lime in potash. The affinity of alumina for lime, I had mentioned in the paper to which I allude; and it has since been noticed by Mr. Vauquelin\*.

Guyton's conclusions, if true, would have rendered analysis uncertain.

If the conclusions of Mr. Guyton had been well founded, it would have been chemically impossible to arrive at truth in analysis. There were already real difficulties enough to be overcome; and Mr. Berthollet has lately discovered some, which are not so easily answered as those I have just considered. The position of this chemist, however, has been too generally extended by him. If the power of masses were as great as he represents it to be, and if it increased *ad infinitum*, in proportion to the mass, it must follow, that, with any given substance, we could decompose any compound, provided the mass of the decomposing body were sufficiently great; but this is well known not to be the case.

Objection to Berthollet's unlimited position respecting attraction from the mass.

From the experiments which I have related, it appears to be proved,

Recapitulation of the affinities of the earths, &c.

- 1st. That there exists an affinity between silica and alumina.
- 2dly. That there exists a very powerful affinity between alumina and magnesia.
- 3dly. That alumina shews an affinity for lime; but that the said affinity is not so strong as Mr. Guyton had supposed, nor, if pure reagents be used, is it to be perceived under the circumstances stated by him.
- 4thly. That Mr. Guyton was mistaken in every instance of affinity between the earths, excepting in the case of silica with alumina, which had been observed before his experiments; and that, in the other cases, he has attributed to a cause which does not exist, phenomena that must have resulted from the impurity of his reagents.

\* Scheele was, in fact, the first who perceived this affinity. See his *Essay on Silica, Clay, and Alumina*.



5thly. That neither the experiments of Mr. Guyton, nor the opinion maintained in the letter from Freyberg, are sufficient to diminish, in any degree, the value of the assistance mineralogy derives from chemical investigation.

## IV.

*A Memoir on the Musical Sounds produced in Tubes by Hidrogen Gas. Read to the Society of Philosophy and Natural History at Geneva. By G. DELARIVE\*.*

IN a former meeting our learned colleague, Professor Pictet, communicated to the Society a series of experiments on the musical sounds produced in tubes by hidrogen gas, in which he developed the various musical phenomena which these tubes produce. He explained the effect which the length or width of the tube, and the situation where the hidrogen is burned, have on the sounds produced. As to the cause of the sound, he gave only a few conjectures; his inquiries not being directed to that object. The purpose of the present memoir is to discover this cause.

Professor Pictet's researches on musical tubes by hidrogen.

Professor Brugnatelli is, I think, the first who published the experiment I have endeavoured to explain †: It was invented by a German. I shall here relate the principal circumstances.

Publication by Brugnatelli.

If a current of inflamed hidrogen gas be inclosed in a tube of an elastic and sonorous material, such as glass, metal, dry wood, &c. the tube after an interval of some seconds, will produce a musical sound. If it is open at both ends, the sound will be strong and full. It is possible however to succeed with a tube hermetically closed at one end, provided its diameter be large enough to allow of a circulation of atmospheric air sufficient to keep the gas in a state of combustion. The conditions essential to the success of the experiment are, *first*, That the tube be elastic and capable of forming an echo, that is to say, capable of reflecting the undulations which proceed from the sonorous point; for no sound will be produced with a tube of paste-board or paper: and, *secondly*, The flame must be produced by

Description of the manner of burning hidrogen in a tube, to produce a musical sound.

The tube must be elastic;

and the flame from hidrogen;

\* Journal de Physique, LV. 165.

† Dr. Higgins discovered it. See Phil. Journal, new series, I.

a current of hidrogen ; for an inflamed stream of the vapours of alcohol or ether, a lighted taper, &c. are incapable of producing any sound.

Circumstances.

The place of combustion is the sonorous point.

Let us now examine the circumstances of the experiment. There must be a point which may be called the *sonorous point* ; at which the vibrations, communicating an undulatory motion to the air, are produced. This point is the place of combustion ; for if this be made to alter its situation, the sounds will vary, as M. Pictet has proved in a series of experiments. He has likewise observed, by means of smoke with which he filled the tube, a continual succession of vibrations at this point ; these vibrations produce the waves, which proceeding with a determinate velocity, strike against the sides of the tube, and are reflected with a velocity equal to that with which they arrived.

Undulations at the point must accord with the resonant echo of the tube.

When the distance of the sides of the tube is such that the alternate reflections are equal to the vibrations natural to the sonorous cause, the sound increases in intensity ; and becomes musically appreciable. It appears likewise that the waves thus reflected do re-act on the primitive vibrations produced at the place of combustion, so as to render them harmoniously regular. For a certain space of time is almost always necessary before the instrument acquires a regular and full sound ; and the tone or pitch of the tube will be more or less acute according to the greater or less number of undulations which take place in a given time.

The sound does not issue at first.

Its tone.

The temperature of the air in the tube is hottest at the burning point, and cooler more remote.

There is another fact essential to be observed in this experiment. The temperature of the column of air is not the same through all its length. At the sonorous point, that is to say the place of combustion, the temperature is very high ; so much so that the end of the adjutage of the glass through which the hidrogen passes, is constantly in a state of ignition. If an inflamed stream of the vapours of alcohol or ether be substituted instead of the hidrogen, the heat is evidently less. From some experiments it also seems probable that the temperature of the room, and the purity of its air, may affect the success of the experiment.

Alcohol or ether give a less degree of heat.

Conjecture that the sound is produced by the rapid production and collapshon of aqueous vapour.

My experiments have been directed to ascertain the cause of these phenomena, how and by what means these sonorous vibrations are produced. We know that water is formed during the combustion of hidrogen : this water appears in the form of vapour. The temperature of the place of combustion being



being very elevated, these vapours must occupy an extensive space; but by immediately coming into contact with a cold air, their volume must rapidly diminish. A vacuum must thus be formed into which the air must collapse, and be again driven by other vapours, which again contract in their turn. Do not the vibrations result from these alternate motions, produced by the great expansion and subsequent contraction of the vapours \*? Such were the conjectures that might be formed as to the probable cause of the phenomenon; but accident has presented me a fact which appears to give them some weight.

I had a thermometer tube of about one line in diameter, at the extremity of which was a small bulb: there was a drop of water which I wished to expel from it; for this purpose I repeatedly exposed the bulb to the flame of a lamp with alcohol, and was agreeably surprised to hear a musical sound proceed from the tube.

Sound produced by heating water in a thermometer bulb and tube.

In order that this experiment may succeed, a tube of one, two, or three lines in diameter, should be taken, of three, four, or five inches in length, with a bulb at one of the extremities, in diameter about three times that of the tube; it need not be very regular. If it were even a little flattened, I think the sound would be louder. A very small quantity of water or mercury must then be introduced, and expose it to a strong

Instructions for making the experiment.

\* It appears probable to me, that the sound produced by air precipitating itself into a vacuum, is more intense than that resulting from an expansive force. The horrible noise occasioned by the detonation of soap bubbles of hydrogen and oxygen is well known, though the lightest objects surrounding the basin are not even shaken. Whence it may be concluded, that the phenomenon is produced by the sudden vacuum proceeding from the destruction of the gases. The detonation of a pistol with inflammable gas is much greater than that of an air-gun, though the effect is less considerable; probably because a vacuum in the pistol succeeds the first expansive force. The child's toy, called the *humming top*, is well known. It is a hollow sphere, with an opening at its circumference: it produces a very strong humming noise when spinned rapidly on its axis. What may be the cause of this humming? I think it is the same as that which I have just mentioned; the centrifugal force drives the air from the sphere through the opening; a kind of vacuum is made, into which the external air continually presses, and is constantly driven back; whence proceed a series of sonorous oscillations.—D.

The sound of air falling into a vacuum much more intense than that from explosion.

The humming top.

heat; that of a lamp with alcohol is commonly sufficient; but the flame must be large and strong when the tube is large. After the tube has been thus exposed for a few seconds, the sound is heard. Those tubes whose diameters are considerable give a deep sound, and the size of the bulb likewise seems also to contribute to the same effect: the sound continues for several seconds, and then gradually decays, and at length entirely ceases. By suffering the apparatus to cool, and causing the condensed liquid to descend into the bulb, the experiment may be repeated as often as shall be thought necessary.

Investigation of  
both kinds of  
sound.

Such is the experiment by means of which I think the phenomenon of the musical tubes may be satisfactorily explained. Let us first examine what passes in the tubes with bulbs, with the essential conditions under which they produce their sound, and then endeavour to discover the cause. I will then compare it with the sound produced in tubes with hydrogen, and enquire in what respects the effects produced by these two instruments resemble one another, with the differences they present, and the cause of those differences.

Conditions for  
the tube and  
bulb.  
There must be a  
bulb;

containing wa-  
ter;

- 47 -

or rather mer-  
cury;  
but not ether,  
alcohol, or sulph.  
acid.

The fluid must  
be very small in  
quantity.

The heat must  
be strong, to the  
bulb;  
and the tube  
kept cold.

There must be  
some air in the  
tube.

The conditions essential to the production of sound in tubes with bulbs are, *1<sup>st</sup>*, That the tube should have a bulb. I never could excite sonorous vibrations in a tube simply closed at one end. *2<sup>d</sup>*, This bulb must contain an evapourable liquid. Water succeeds very well, but it has this disadvantage; it forms, in passing from the vapourous to the liquid state, a drop of water in the tube, which often completely obstructs it; and at other times, by running along the warm part of the glass, it frequently breaks it. Mercury has not this inconvenience: I never could succeed in producing sounds with ether, alcohol, or concentrated sulphuric acid. The sounds are influenced by the quantity of liquid contained in the bulb. It should be as small as possible. If there be too much, the vapour fills the tube, and by completely expelling the air, renders it equally warm throughout, and no sound can then be produced. *3<sup>d</sup>*, The third condition essential to the success of this experiment, is the application of a strong heat to the bulb while the rest of the tube continues cold; for if there be not a marked difference in the temperatures of the tube and of the bulb, no sonorous effect will take place. *4<sup>th</sup>*, The presence of atmospheric air in the tube is indispensable; if it be entirely excluded, no effect will be produced; and in every stage of the experiment it



it will be found that the vapour occupies only a certain part of the instrument, and that air is always present. I have frequently endeavoured to determine the exact space occupied by the vapour at the moment the sound was heard, and I found, at least in the small tubes, that it was nearly equal to the volume of the bulb. In order to determine this, I closed the orifice of the tube with my finger at the moment it began to sound; I then plunged the end of the tube in mercury, withdrew my finger, and suffered the apparatus to cool. The vapour thus became condensed, and the space it occupied was estimated by the quantity of mercury which the pressure of the atmosphere caused to rise in the tube.

Such are the four conditions essential to the production of sounds; namely, a bulb at the extremity of the tube; the presence of a very small quantity of water or mercury in this bulb; the application of a strong heat to it, whilst the remainder of the tube remains cold; and, lastly, the simultaneous presence of atmospheric air and vapour in the apparatus; it is scarcely necessary to add, that the orifice of the tube should be always open. Let us now consider what may be the cause of the sound.

I first endeavoured to determine whether any chemical decomposition of the liquid employed took place. For this purpose I used a tube sufficiently long to permit all the liquid to condense in it; I weighed it carefully before I subjected it to experiment, and found that its weight was neither augmented nor diminished after repeated productions of sound: Whence I conclude, that the caloric has no chemical effect on the liquid, but that it only experiences successive evaporations and condensations. Must we then attribute the sound to the evaporation, and condensation of the liquid? I thought so at first, but the following considerations made me alter my opinion: I first observed that it was possible to have a successive evaporation and condensation of the liquid, without producing sound: this was effected by applying heat not sufficiently intense to produce sound. Secondly, When the experiment was made with a drop of water, I always found that the moment when the apparatus began to sound, was that at which the whole fluid had evaporated, and consequently that the caloric acts on the vapour alone. While the smallest atom of liquid water remained in the bulb, no sound was produced. From this fact I have deduced, that the sound is produced by the

Proof of this.

Recapitulation.

The sound is not produced by chemical change;

nor by evaporation and condensation;

for these last may happen without sound;

action



but to a tremulous expansion and contraction of the vapour; which thus strikes the air and produces sound.

action of caloric on the vapour and its re-action on the atmospheric air. I conceive that this phenomenon takes place in the following manner: The vapour contained in the bulb, by means of an addition of caloric applied on all sides and in great quantity, acquires an augmentation of volume and elasticity; it rapidly passes from the bulb into the tube, and drives out the air contained in it; but this air and the sides of the tube cause it again to lose a portion of its caloric, and its volume immediately diminishes. A vacuum is thus made, and the air resumes its primitive situation. Another addition of caloric restores its elasticity, which it speedily loses in the same manner. A series of oscillations by this process give the air an undulating motion; and the waves being reflected by the sides of the tube, become sonorous and appreciable as soon as their progress and recess are isochronous with the oscillations produced

It seems as if the expansions of the vapour ought to be isochronous with the pitch or action of the tube governed by its length and diameter. These sounds cease as soon as the tube has become too hot to occasion the sudden contraction of the vapour.

Experiments in confirmation.

by the cause here pointed out. There are some tubes which never produce any sound: In this case I presume that the advance and recess of the waves cannot harmonize with the primitive oscillations, but that the one destroys the other. After a certain time the sound diminishes and dies away: This is explained by the propagation of the heat along the sides of the tube when the bulb is very warm and the tube cold. The vapour when first driven from the bulb, *suddenly* loses a part of its volume, and the oscillations thus produced are strong and frequent; but when the tube has acquired a certain degree of heat, the vapour then *gradually* diminishes in volume as it passes from a very high temperature to a situation certainly less warm, though of a sufficient degree of heat to cause the oscillations to become weaker, and at length entirely to cease. That this is the cause of the cessation of the sound, may be shewn by applying a strong heat to the part of the tube already warmed, and at the same time keeping up the first degree of heat at the bulb. By this means the limit of temperature is again precisely determined, and the sound reproduced in all its force. It may easily be conceived that the substance of the tube ought to be a non-conductor of heat, and on this account glass is preferable to any other substance.

Concerning the sounds produced by hydrogen. Repetition of the theory already stated.

Let us now compare the tubes which sound by means of hydrogen with those with bulbs. In the former we have every condition necessary to the production of sound; a very hot vapour, and consequently very elastic; for as we observed, the

the period of combustion is at so high a temperature, that it always ignites the tip of the glass tube; this hot elastic vapour, at the moment of its production, comes in contact with the cold air, which enters through the bottom of the tube and goes out at the top. Its volume must be diminished the instant it strikes this air; other heated vapours succeed the former, and are also contracted: this alternate expansion and contraction produces the undulatory motion of the air, and causes the sonorous waves.

Expansions and  
collapsions of  
aqueous vapour  
near the point of  
combustion.

We have found that an inflamed stream of alcohol or ether produced no sound in a tube. This is a proof of my observation, that in order to produce sound, there must be a great difference in the temperature of the vapour and the ambient air. There is certainly, in this case, a formation of vapours and successive condensations, for the water runs along the sides of the tube; but the point of combustion is infinitely less hot than that of hydrogen, consequently the vapour produced is much less heated and elastic. This case is similar to that formerly mentioned, where it was shewn that vaporization and successive condensation of the liquid might be effected without sonorous undulation, merely by exposing the bulb to a certain degree of heat not intense enough for this last purpose. We cannot be surprised that there is less heat produced by the combustion of alcohol or ether than by that of hydrogen, if we consider, that in the latter case all the caloric contained in the gas and the atmospheric oxygen which is consumed, becomes sensible heat, and is passed totally into the vapour produced. On the contrary, in the combustion of an inflammable substance, such as alcohol, the caloric of the oxygen consumed alone becomes sensible, and this also is mostly absorbed by the production of carbonic acid gas; so that the excess alone passes into the vapour. It is not therefore surprising that the heat is not sufficient to give the vapour elasticity sufficient to produce sounds. The presence of carbonic acid gas, which is one of the results of the combustion, may also prevent the sonorous vibrations.

Alcohol and  
ether burn with  
less heat, and  
produce elastic  
fluid which is  
neither rapidly  
nor totally con-  
densed.

Why there is  
less heat.

The sound is much stronger in the tubes with hydrogen, than in those with bulbs. It is likewise more permanent; for the tube being open at both ends, a current of cold air constantly enters at the bottom and issues out at the top: this current of air takes up and carries off the hot elastic vapours, receives their impulse, and by attracting a portion of their caloric, diminishes

The sound by  
hydrogen is  
strong and per-  
manent, because  
the constant  
supply of cold  
air produces the  
condensations  
incessantly.



Not so in the  
bulbs.

minishes their volume. This then is the most essential condition to the production of an intense permanent sound; namely, a great difference between the temperature of the air and the vapour; and this difference remains always the same, on account of the continual renewing of the air. The same effect does not take place in the tubes with bulbs, and accordingly their sound is weaker and less durable.

The sounds by  
hydrogen are less  
intense in a  
warm room filled  
with company;

From the knowledge of this condition, that a great difference between the temperature of the air and the vapour is necessary to the production of sound, it will be easy to comprehend that every circumstance which augments the heat of the current of air, and diminishes that produced by the combustion of the gas, will tend to weaken and even to annihilate the sound of the tube. These two circumstances are both found in a warm room filled with company. The current of air instead of being cold is heated, and the quantity of oxygen being less, the heat of the combustion is less. It is not then surprising that in such rooms the experiment does not always succeed.

because the air is  
warmer, and the  
combustion  
weaker for want  
of oxygen.

Sounds by phos-  
phorus.

Brugnatelli produced sounds by the simple combustion of phosphorus in tubes. Some philosophers being persuaded that sonorous effects could only be attributed to hydrogen, were disposed to infer its presence in phosphorus. From what we have stated, it seems more simple to explain the phenomenon by the production of phosphoric acid in the vaporous form, which becomes strongly elastic by means of the caloric disengaged during the combustion, but of which the volume diminishes by the contact of cold air. We have here the alternate expansion and contraction necessary to the production of sound.

Conclusion.

Such are the small number of observations which I have had an opportunity to make on the sonorous tubes. I hope that they will interest such as are more particularly engaged in this branch of natural philosophy, and tend to direct their attention to a curious fact which has not hitherto been sufficiently examined.

#### ANNOTATION.—W. N.

That aqueous  
vapour is con-  
densed with  
great rapidity;

AS the fundamental effect in this very ingenious explanation (namely, that aqueous vapour is condensed with extreme rapidity), may not appear to some readers to be  
fo



so speedy as to produce sonorous undulation, I think it will not be impertinent to mention a few facts relating to that subject. In the small glass instrument called the water-hammer, which is a tube of about three quarters of an inch in diameter, with a ball of about an inch and half at one end, the other end being hermetically closed; the ball contains water, and the empty space is rendered nearly vacuous by boiling the fluid previous to sealing it. In this instrument the heat of the hand, applied to the wetted tube, is sufficient to produce bubbles of vapour which enter the ball, but speedily collapse. The series of these condensations is as quick as fifteen or sixteen in a second. But in the steam engine the condensation is prodigiously more rapid. I have a small double steam engine on the construction of Boulton and Watt, having all the parts and gear of the large engines, but its cylinder is only  $2\frac{1}{2}$  inches diameter, and the length of stroke  $6\frac{1}{4}$  inches. When this was set to work the other day, in a lecture to my pupils, it gave 600\* strokes per minute. By an easy calculation it will be shewn, that the steam condensed was then much more than 300 cubic inches per second; and if the condensation, instead of being effected in masses of about a pint at a time, could have been performed by successive collapses of each cubic inch in an open space, the pulses would have produced the tone of the lowest E flat in the treble cliff. But the number of cubic inches condensed in a large steam engine, for example, a three feet cylinder with an eight feet stroke, will be eight or nine times as much, at the usual rate of working.

\* About twice as many as the beats of a common watch.

## V.

*Account of a simple Method of estimating the Changes of Volume produced in Gases, by Alterations of Temperature, and of Atmospheric Pressure, in the course of Chemical Experiments. By MR. H. DAVY. Professor of Chemistry at the Royal Institution. From their Journal.*

Elastic fluids affected by pressure and temperature.

IT often happens that changes of atmospherical pressure, and of temperature, take place in the course of experiments on elastic fluids; and a knowledge of the alterations they produce in their volume, is essential to the precision and accuracy of the results.

Quantities of gases must be estimated at some standard.

In cases when chemical changes are produced, it is impossible to gain this knowledge by direct observation; and, in considering quantities, it is always useful to estimate the volumes of gases at some standard, fixed upon in measures of the barometer\* and thermometer.

Law for pressure. Volume, inversely as pressure.

It is demonstrated by very accurate experiments, that the volumes of elastic fluids are inversely as the weights compressing them. And, consequently, the changes produced upon gases, by known changes in the atmospherical pressure, may be ascertained in a very easy manner. With regard to the effects of temperature, however, it is much more difficult to form a just estimation by means of general laws. For though the excellent experiments of Mr. Dalton †, and those of

\* Lavoisier's Elements, p. 406, 2nd Edit.

† Manchester Memoirs, Vol. V. p. 599. Mr. Dalton says, "I have repeatedly found that 1000 parts of common air, of the temperature of 55° and common pressure, expand to 1321 parts in the manometer; to which adding 4 parts for the corresponding expansion of glass, we have 325 parts increase upon 1000 from 55° to 212°; or for 157° of the thermometric scale. As for the expansion in the intermediate degrees, which Col. Roi's experiments show to be a *slowly diminishing* one above the temperature of 57°, but which de Morveau's, on the contrary, show to be a *rapidly increasing* one in the higher part of the scale; I am obliged to allow that Col. Roi is right, though it makes, in some degree, against an hypothesis I have formed relative to the subject; he has certainly, however, made the diminution too great from 72° downwards, owing

of Mr. Gay Lussac, show that all the different elastic fluids, taken at equal temperatures, expand equally by heat, yet we are still ignorant of the precise expansion for increments of single degrees, or of the mode in which the power of expansion is affected by difference of pressure; or by its relations to aqueous vapour or uncombined moisture.

The calculations on this subject are consequently laborious and complicated; but it is very easy to avoid them, by recurring to comparative observations, which may be obtained in a very simple manner, by means of the manometer.

By making a given quantity of air, in contact with water at a known temperature and pressure, a standard, it is easy, in ascertaining the changes produced in it by alteration in the temperature and pressure, to determine, by the rule of proportion, the changes that have been produced from the same causes in any other quantity of gas, submitted to chemical operation; or to ascertain what would be its volume at the mean height of the barometer and thermometer.

owing to his not perceiving that he actually destroyed a portion of the elastic fluid he was operating upon (aqueous vapour) in reducing its temperature so low; if his air had been previously dried by sulphuric acid, &c. he would not have found so remarkable diminution below  $72^{\circ}$ . My experiments give for  $77^{\circ}\frac{1}{2}$  above  $55^{\circ}$ , 167 parts; for the next  $77^{\circ}\frac{1}{2}$  only 158 parts: and the expansion in every part of the scale seems to be a gradually diminishing one in ascending.

“ The results of several experiments made upon hydrogenous gas, oxygenous gas, carbonic acid gas, and nitrous gas, which were all the kinds I tried, agreed with those on common air, not only in the total expansion, but in the gradual diminution of it in ascending: the small differences observed never exceeded 6 or 8 parts on the whole 325: and differences to this amount will take place in common air, when not freed from aqueous vapour, which was the situation of all my factitious gases.

“ Upon the whole therefore I see no sufficient reason why we may not conclude, that *all elastic fluids under the same pressure expand equally by heat*—and that *for any given expansion of mercury, the corresponding expansion of air is proportionally something less, the higher the temperature.*”

Mr. Gay Lussac makes the dilatation to be from 100 to 137.5 between  $32^{\circ}$  and  $212^{\circ}$ , which gives for each degree between these points  $\frac{1}{493}$ . See p. 134; and *Annales de Chimie*, No. 128, p. 137.



confined in a  
recurved tube by  
water in the  
bend;

graduated

the water kept  
level, by a  
plunger.

Use of the  
standard of  
comparison.

Instance.

It is most pro-  
bable that the  
results are not  
disturbed by the  
aqueous vapour.

In forming the manometer for the purpose of comparison, a glass tube may be used about sixteen inches long, and one third of an inch in diameter, closed at one end, and curved in such a manner that its open leg is parallel to its closed end, and nearly three inches long. Its capacity is determined, and its closed leg is graduated so as to form a scale of 200 parts. The standard volume of air is confined in it by a column of water, four inches long; and the height of this water is kept equal in both legs at the different times of observation, by means of a glass tube, moveable in a perforated cork inserted in the open end, and capable of elevating the column of water in it at least an inch and a half.

In employing the standard of comparison, for correcting the results of operations in which portions of elastic fluids are either absorbed or generated, it is only necessary to recur to the state of it at the beginning and end of the experiment. Thus, let  $n$  equal the quantity of elastic fluid existing after the experiment;  $m$  the volume of the standard air before the experiment; and  $v$  the volume after, as expressed in the scale of 200 parts.

Then  $\frac{nm}{v} = x$ , which is the volume the residual gas examined would occupy at a temperature and pressure such as existed at the commencement of the experiment.

By the same method may be estimated the volume a quantity would occupy at such temperatures and pressures, as had been at any time denoted upon the scale.

From the latest experiments that have been made\*, it is probable that at the same temperatures, and under the same pressures, equal volumes of the different elastic fluids, in contact with water, contain the same quantity of aqueous vapour; so that in cases when the gases examined in the comparative observations are equally saturated with water, the results must be perfectly accurate as to the relation of volume to the state of moisture; and, even supposing a difference in the degree of saturation, the error arising from this circumstance, at common temperatures, would be so small as to be inappreciable.

\* Those of Saussure and De Luc, and MM. Deformes and Clement.

## VI.

*Observations on Sir George Shuckburgh Evelyn's Paper\* in the Philosophical Transactions for 1798, on the Standard of Weight and Measure. By J. FLETCHER, Esq. Communicated by the Author.*

I. **H**AVING about two years ago had occasion, in the course of some investigations in which I was then engaged, to ascertain with as much precision as was possible the relation between the known measures of distilled water and their respective weights at different temperatures; Sir George Shuckburgh Evelyn's Paper on this subject was of course considered as the best authority with regard to what had been previously done in this respect. I found however, on looking into it, some errors in the calculations, which render the deductions from them incorrect, at the same time that they more strongly confirm and establish the accuracy of the experiments on which they are founded. I have no doubt, therefore, that the gentleman to whom we are so highly indebted for the communication of the facts which he states in this important essay, will feel rather obliged than otherwise by their correction. The circumstance which first induced me to repeat the calculations, was the difference which there appears to be in the results of the experiments with the cube and sphere, and that with the cylinder, and which Sir George ascribes, in § 24 and 31 of his Paper, to either a difference in the density of the water at different depths, or to the compressibility of the cylinder employed. I was however as much pleased as surprized to find, that it altogether arises from a mistake in the multiplication of the four numbers whose product is equal to its solid content, viz.  $3.99745 \times 3.99785 \times 5.99502 \times .78539816$ , which he makes 74.94823 cubic inches, instead of 75.247149, which is the real product. This naturally suggested the propriety of examining the whole, and the results of this examination are what I shall now lay before the public through the medium of the Philosophical Journal. The power of correctly appreciating and comparing space and quantity are so essential to

Occasion of this essay.

Numerical errors in Sir G. S. Evelyn's paper.

Apparent difference in results.

Mistake in the solid content of his cylinder.

\* Copied in the Quarto Series of this Journal, III. 197.

every practical application of the mathematics to the purposes of real utility, that I have no hesitation in undertaking the humble office of commentator on so valuable a text.

Real cubic dimensions of Sir G. S. Evelyn's solids.

II. It is stated in § 13\* that the product of the three mean measures of the sides of the cube, *viz.*  $4.98882 \times 4.98955 \times 4.98925$ , is 124.18917: this however is erroneous, it being in fact 124.192246. The solid content of the cylinder was 75.247149, as already stated in the preceding paragraph, and that of the sphere (§ 28) =  $\overline{6.00745^3} \times .52359878 = 113.519147$  cubic inches.

Correction for temperature of the brass.

III. Supposing the brass scale and these bodies to be at the same degree of heat, their apparent dimensions as measured by it would be the same whatever was the temperature of both; the scale being of the same substance, and consequently expanding and contracting with the bodies themselves. Their real dimensions however varying with the temperature, we must assume some certain degree of heat at which the graduations on the scale are to be considered as measuring correspondent intervals of space. We will suppose this to be 60° of Fahrenheit's thermometer. Now in order to ascertain the real cubic measure by the scale when at 60°, of the quantities of distilled water displaced by those bodies when they were weighed in it, we must add to or subtract from the nominal cubic dimensions of each, 3 millionth parts (*vide* Tab. I. in the note on § 23), multiplied by the number of degrees above or below 60°, at which they and the fluid were at each of those times.

At the time of their being respectively weighed in water, therefore, the solid content of the cube at 60.2° (§ 22) was  $124.192246 \times 1.00000062 = 124.192323$  cubic inches of the scale when at 60°; that of the cylinder at 60.5° (second experiment, § 23)  $75.247149 \times 1.0000015 = 75.247161$ ; and that of the sphere at 66° (§ 29)  $113.519147 \times 1.000019 = 113.521304$ .

Correction necessary for density and temperature of the air.

IV. In appreciating the apparent difference of weight in a body when weighed in air or in vacuo, it has been usual to assume, that air at a given temperature, and under a given

\* These references with a § prefixed, are to the sections so numbered in Sir G. S. Evelyn's Paper, which it is thought proper to mention here to save the necessity of repetition.



pressure, is always of the same specific gravity. This we know is not the case; considerable variations in this respect being occasioned by its different states of dryness, electricity, and perhaps other causes: as, however, experiments are wanting for the correct estimation of these differences, we will take the first supposition to be true, and apply the corrections accordingly from Sir G. S. Evelyn's tables for this purpose, in his note subjoined to § 23.

This gentleman tells us in the Philosophical Transactions for 1777, that the specific gravity of air at  $51^{\circ}$ , under a pressure equal to 29.27 inches of mercury, is to that of water as 1 to 836, or  $= .001208$ ; taking the latter as unity. Assuming therefore that the expansion of air, for each degree of Fahrenheit's thermometer between  $51^{\circ}$  and  $60^{\circ}$ , is .0027 of the whole, we get the specific gravity of air at 60 under the like pressure  $= .001179$ , and  $29.27 : .001179 :: 29.5 : .001188$  Air expands .0027 for  $1^{\circ}$  of Fahrenheit.  
 $=$  the specific gravity of air at  $60^{\circ}$  when the barometer stands at  $29\frac{1}{2}$ ; and a cubic inch of such air consequently weighs  $\frac{3}{10}$  of a grain very nearly. Sp. gr. of air at  $60^{\circ}$ , bar. 29.5,  $= .001188$   $=$  .3 gr. for each cubic inch.

Now the cube was weighed in air at  $62^{\circ}$  under a pressure of 29 inches (§ 20); the cylinder in air at  $62^{\circ}$  under a pressure of 29 inches (§ 21); and the sphere in air at  $67^{\circ}$  under a pressure of 29.74 inches (§ 29). Densities of the air in which the bodies were weighed.

And the excesses of the weights of the respective quantities of distilled water displaced by these bodies, on their being afterwards weighed in this fluid, over those of similar quantities of air at the several temperatures and densities above-mentioned, were as follow; viz.

That of the water displaced by the cube (§ 22),  $32084.82 - 703.03 = 31381.79$  grains; The apparent weights of equal bulks of water.

That of the water displaced by the cylinder (second experiment § 23),  $21560.05 - 2553.22 = 19006.83$  grains;

And that of the water displaced by the sphere (§ 30), 28673.51 grains.

The air in which each of the bodies was weighed being, however, lighter than air at  $60^{\circ}$  when the barometer stands at  $29\frac{1}{2}$ , we must correct these excesses of weight in the several quantities of water, so as to find how much the weight of each of them exceeds that of an equal quantity of air of such standard specific gravity; or in other words, how much the weight of each of the bodies in water differs from that which Correction for pressure and temperature applied.

it would have been found to possess in air in this latter state. These corrections for the density and temperature of the air we shall find, from the data before given in this article, to be as follows :

	Correction for	
	Pressure.	Temperature.
	Gr.	Gr.
Of the weight of the water displaced by the cube - - -	- .642	- .201
Of the weight of the water displaced by the cylinder - - -	- .389	- .122
Of the weight of the water displaced by the sphere - - -	+ .282	- .644

Correction of sp.  
gr. of water  
when reduced to  
60°.

The weights of these bulks of water, therefore, if weighed in air at 60° under a pressure of  $29\frac{1}{2}$ , would be found to be,

That displaced by the cube  $31381.79 - .642 - .201 = 31380.95$  grains ;

That displaced by the cylinder  $19006.83 - .389 - .122 = 19006.32$  grains ;

And that displaced by the sphere  $28673.51 + .282 - .644 = 28673.15$  grains.

V. We have now, however, to correct these weights for the change which would take place in the specific gravity of the water itself, by reducing it to 60° of temperature ; that in which the cube was weighed having been at 60.2° (§ 22) ; that in which the cylinder was weighed at 60.5° (§ 23) ; and that in which the sphere was weighed at 66° (§ 30).

Firstly, by Sir  
G. S. Evelyn's  
table,

We will firstly do this by the help of Sir G. S. Evelyn's Tab. I. in his note on § 23, according to which, a quantity of water of equal bulk with the cube would, at 60° of Fahrenheit's thermometer, weigh  $31380.95 \times 1.000033 = 31381.986$  grains ; one of equal bulk with the cylinder,  $19006.32 \times 1.000083 = 19007.897$  grains ; and one of equal bulk with the sphere,  $28673.15 \times 1.00099 = 28701.536$  grains ; if weighed in air at the same temperature, when the barometer stands at 29 inches and a half.

The first gives the weight of a cubic inch of distilled water under these circum- stances = $\frac{31381.986}{124.192323} = 252.689$	Grains by Sir G. S. Evelyn's Weights, or	252.523	Grains of the Parliamentary Standard Weights, which are those used by him as 1.0006562 : 1. (§ 41).	Result.
The 2d = $\frac{19007.897}{75.247161} = 252.606$				
And the 3d = $\frac{28701.536}{113.321304} = 252.829$				

VI. It may perhaps however be proper to observe here, By Mr. Gilpin's tables: that the alteration in the specific gravity of water by change of temperature, is estimated in this table of Sir G. S. Evelyn's (note on § 23), at nearly double that which is given by Mr. Gilpin in his tables, in the Philosophical Transactions for 1794, p. 382; his appreciation of it from 60° to 66° (taking it to be equal to unity at 60°) being as follows; viz.

61°	.99991	63°	.99971	65°	.99950
62	.99981	64	.99961	66	.99939

I am, from my own experiments on this subject, disposed to prefer these tables; and taking them to be correct, we shall have a cubic inch by Sir G. S. Evelyn's scale when at the tem- Ultimate result.  
perature of 60°, of distilled water also at 60°, weighed in air A cubic inch of  
at 60°, under a pressure of 29½ inches. water at 60°, in  
air at 60°, bar.

By the cube =	-	252.519	Grains of the Parliamentary Standard ac- cording to Sir G. S. Evelyn's appreciation of it in § 41 of his Paper.	29.5, = 252.506 grains;
By the cylinder =	-	252.432		
By the sphere =	-	252.568		
And by a mean of all three =		252.506		
And if weighed in vacuo instead of in air =		252.806		or = 252.806 grs. in vacuo

J. FLETCHER.

Cecil Street, Dec. 20, 1802.



## VII.

*On the Quantities of Light afforded by Candles in Proportion to the Consumption of Material and other Objects respecting the same. In a Letter from MR. EZ. WALKER.*

To Mr. NICHOLSON.

SIR,

Probability  
that Candles  
which burn  
without pro-  
ducing smoke,  
will afford light  
in proportion to  
the matter con-  
sumed.

WHEN a lighted candle is so placed as neither to require snuffing nor produce smoke, it is reasonable to conclude that the whole of the combustible matter which is consumed is converted to the purpose of generating light; and that the intensities of light generated in a given time by candles of different dimensions, are directly as the quantities of matter consumed. That is to say; when candles are made of the same materials, if one candle produce twice as much light as another, the former will in the same time lose twice as much weight as the latter.

Experiments in  
proof.

To prove the truth of this position, I made the experiments contained in the following

TABLE.

No. of the Experiment.	No. of the Candles.	Time of burning.	Weight of the Candles consumed.	Strength of Light.	Distance of the Candles from the Wall.
		h. '.	oz. dr.		Feet.
1	1	3 0	0 15	1.	7
	3	3 0	1 1 $\frac{1}{2}$	1. +	7
	Mould	3 0	0 15	1.	7
2	1	2 55	0 15	1	8
	3	2 55	1 0	1 +	8
	Mould	2 55	0 15	1	8
2	1	3 0	0 15 $\frac{3}{4}$	1	8
	3	3 0	1 2	1 $\frac{1}{8}$	8 $\frac{3}{4}$
	Mould	3 0	1 0	1	8
4	5	3 0	1 5	1.18	8 $\frac{3}{4}$
	Mould	3 0	1 1 $\frac{1}{8}$	1.	8

These

These experiments were made in the following manner.

I took three candles, the dimensions of which are given in the table, against 1, 3, and mould \*. These were first weighed, and then lighted at the same instant. At the end of the time inserted in the third column of the above table, they were extinguished and weighed again, and the loss of weight of each candle is contained in the fourth column.

The three first experiments were made under such favourable circumstances, that I have little doubt of their results being more accurate than what practical utility requires, but the fourth experiment cannot be depended on so much, in consequence of the variable light of No. 5. This candle was moved so often to keep the two shadows equal, that I was under the necessity of setting down its mean distance from the wall by estimation; but as this was done before the candles were weighed, my mind could not be under the influence of partiality for a system.

The method which I used in comparing one light with another in each experiment, was nearly that which has been already described †. The only difference consisted in having the obstacle which formed the shadows fixed, instead of being held in the hand. A two feet navigation scale, which was made fast to the stand of a small telescope in a perpendicular direction, and set upon a table near the wall on which the shadows were compared, reduced the labour of making the experiments very much, and gave me an opportunity of making a greater number of observations in the same time than I could have done with the scale in my hand.

## REMARKS ON THE EXPERIMENTS.

1. The experiments were made at different times, and the light of the mould candle was made the standard, with which the lights of the others were compared; but it must not be understood, that this candle gave the same strength of light in every experiment.
2. The sign + in the 5th column, signifies that the candle against which it is placed, gave a stronger light than the others

The standard comparison of experiments at different times was a mould candle by the weight consumed, of which the lights were inferred.

Other remarks.

\* Philosophical Journal, octavo, III. 273.

† Philosophical Journal, quarto, I. 67. and Philos. Journal, octavo, III. 275.

in the same experiment, but the exact quantity being small, I did not ascertain by calculation.

3. The mould candle in the 4th experiment, lost more of its weight in three hours, than in the preceding experiments. This was owing to the current of air in the room in which the experiments were made, being greater than usual, occasioned by a fresh gale of wind.

### GENERAL LAWS.

General law as at first stated:

From the experiments contained in the table, it appears to be an established law, where combustion is complete, that the quantities of light produced by tallow candles, are in the complicate ratio of their times of burning and weights of matter consumed.

For if their quantities of matter be equal, and times of burning the same, they will give equal quantities of light, *by the experiments.*

And if the times of burning be equal, the quantities of light will be directly as their weights of matter expended, *by experiments 3 & 4.*

Therefore the light is universally in the compound ratio of the time of burning and weight of matter consumed.

### STANDARD OF LIGHT.

More ample explanation respecting the standard candle,

If the law which I have endeavoured to prove, both by reason and experiment, be admitted, we have a standard by which we may compare the strength of any other light.

Let a small mould candle, when lighted, be so placed as neither to produce smoke nor require snuffing, and it will lose an ounce of its weight in three hours. Let this quantity of light produced under these circumstances, be represented by 1.00.

Then should this candle at any other time, lose more or less of its weight in three hours than an ounce, the quantity of light will be still known, because the quantity of light in a given time is directly as the weight of the candle consumed, *by the general law.*

### CONCLUSION.

Advantages of the inclined candle;

A candle which is used in the manner that I have pointed out, gives *more light* than a candle of the same dimensions set perpendicularly



perpendicularly and snuffed, because one part of a candle that is snuffed is thrown away, and another part flies off in the form of smoke. But this is not the only inconvenience that attends the using of a candle in this manner, and which the other method is free from, for the light which it gives is of a bad quality, on account of its being variable and undulating.

From the time that a candle is snuffed till it wants snuffing again, its strength of light scarcely continues the same for a single minute. But that variation which frequently takes place in the height of the flame, is a matter of still more serious consequence.

The flame of a long candle, when it burns steadily, is about two inches high, but it very frequently rises to the height of four inches or upwards; drops down again in a moment, till it is less than three inches, and then rises again. In this manner the flame continues in motion for some time before it returns to its original dimensions. But it does not continue long in a quiescent state before it begins a new series of undulations. In this manner the candle burns till the top of the wick is seen near the apex of the flame, carrying off clouds of smoke. In this state of things the eye becomes uneasy for want of light, and the snuffers are applied to remove the inconvenience.

It is these sudden changes, and not the nature of candle light itself, that do so much injury to the eye of the student and artist; but that injury may be easily prevented, by laying aside the snuffers, and in the place of one large candle, let two small ones be used in the manner which I have before taken the liberty to recommend.

I remain,

SIR,

Your's respectfully,

EZEKIEL WALKER.

*Lynn Regis, Dec. 20, 1802.*

## VIII.

*Description of an Engine for raising and lowering Weights by the Action of a Column of Water and for other purposes. By JOHN HARRIOTT, Esq.*

To Mr. NICHOLSON,

Dear SIR,

I SEND the drawings and descriptions of my syphon engine, which from its power and great convenience in actual practice, will I presume be thought a fit subject for your valuable publication.

Engine acting by a column of water acting alternately on the upper and under surface of a piston, as directed by turning a cock.

A A. In Fig. 1 and 2, is a cylinder with a moving piston therein, of which D is the piston rod.

B and C. Are water ways through which the water is admitted to communicate with both sides of the piston.

E F. A pipe in Fig. 1, through which water descends from a reservoir above, into a three way cock M. and in Fig. 2, is a pipe through which any stream or head of water runs to the three way valve in the cistern M.

C H. Is a pipe in both, communicating from the three way cock, or valve, to the upper part of the cylinder.

K B. Is a pipe communicating from the same cock or valve, to the lower part of the cylinder.

I I. Is a pipe communicating between the two last mentioned pipes, consequently between the upper and lower spaces of the cylinder, which communication can be either cut off or opened to any requisite degree by the cock L.

N. Is a pipe in which a lower column of water is suspended by the reaction of the atmosphere, and consequently a power to the upper column, or fall in proportion to its length or depth, not exceeding the weight of the atmosphere.

The nature and principle of the syphon engine consists in combining the power of the syphon with the direct pressure of a column or stream of water, so that they may act together. It works by means of the syphon constantly acting in concert with the feeding stream of water, so that each alternately act on the upper and lower part of a piston, within a cylinder as it were, reversing the syphon at each change; and the power is equal to a column of water of the same diameter as that of the

The column consists partly of an actually pressing mass and partly of a suspended portion, which renders the atmosphere active on the machinery.

the cylinder, and equal in length to the height of the head, above the tail water. For instance, if a column of water of any given diameter has a fall of 20 feet until it reaches an engine, its power is clearly ascertained. Now whatever that power is, if a syphon pipe be added to this engine, so as to connect with the column, and the syphon pipe has also a fall of an equal length; namely, 20 feet to the lower end, which is immersed in water, the engine although placed in the midway, will then have a power equal to that of a descending column of 40 feet, and should the column or fall to the engine be but two feet, and the lower syphon pipe 24 feet, the power would be equal to a fall of 26 feet; and in this manner in every various diversity between the falling column and the syphon pipe beneath, the latter will produce an equal power according to its proportionate length, or depth to the surface of the tail water, provided it does not exceed above 30 feet, or the weight of the atmosphere; and where a stream of water is either level with, or even below, the place at which it is desirable to fix the engine, there will be no difficulty in placing it either below, or on the level, or above the stream itself, provided the height where it is fixed above, does not exceed 28 or 30 feet, and the place where the water flows off be still lower. The construction may evidently be varied according to the local situation and circumstances of applying it, and the use to which it may be adapted, in giving activity to different kinds of machinery.

The drawing, Fig. 1, exhibits the apparatus for raising or lowering weights of any kind, on wharfs or in warehouses. A man or boy can raise or lower goods of any weight, without other exertion than that of merely turning the three way cock M. to an index; in either raising or lowering, the stop is instantaneous, by a small motion or turning the cock to the stop mark in the index: this most effectual of stops, or gripe, operates so quietly and easy without any jirk, or jarring, that it removes the usual risk attending common cranes or machinery in which men are sometimes overpowered. It raises and lowers goods with thrice the velocity usually produced by manual labour, yet an engine of dimensions sufficient to raise several tons, may be so graduated by the person at the cock, as to bring it to the smoothest slowest motion possible. The saving of labour and time must therefore be considerable, the

Engine to be used instead of a crane. Its advantages stated.



risk of plunder diminished, and delays in setting to work for want of help removed.

Though this engine requires a reservoir of water; yet this may easily be had and will be highly beneficial in cases of fire.

The great and only obstacle to its general application in this way, is the want of a natural head of water in most situations of warehouses; and the question in these cases would be, whether it would be worth while to raise a head or reservoir of water for such purposes. If this be done, *one* reservoir would work any number of engines at any distance by one main pipe, and as many branch pipes as there were engines. The expence of a small steam engine to raise the same water (or any other) to the reservoir, would be more than repaid by the security it would give against fire. A gallon of water at the first alarm of fire, has more effect in extinguishing it, than a hog'shead after it has got a-head. To produce this effect, and apply it in the speediest manner wherever such reservoir is, would only require small pipes to be led from the main pipe that supplies the engines, to the warehouses, or where it may otherwise be wanted, and at the end of such pipe, a screw nozzle and a cock to turn on or off, a leather hose and branch pipe, such as all fire engines have, being provided and hanging near. One man only at the first alarm of fire, would in a few minutes screw the hose upon the screw nozzle, and then turning the cock, he would of himself be able to play upon the fire with all the force of a strong fire engine.

Proposal for public reservoirs of water in every parish.

Perhaps it is not too much to say, that this ready preventative in case of fire, would be equal in effect to an insurance. Independent therefore of its application to the syphon engine for raising and lowering weights, I have conceived, that if such reservoirs of water were provided in all cities and towns, well elevated at suitable distances (one or more in every parish) there would not be near the danger and calamity from fire that now exists. The distance of the reservoir from the fire is of no consequence, provided the pipes were laid with sufficient water way; and it would seldom happen that more than one fire at a time would break out in the same parish, or if it did, the same speedy and effectual assistance could be given by any number of such powerful and self-working engines, as long as there was water in the reservoir. And even supposing the supply from the reservoir to be limited to any given time of expenditure, common prudence would lead to its being replenished by the usual mode, before it could be entirely exhausted.

The

The drawing Fig. 2, shows how the syphon engine is to be applied to streams of water, the advantages of which are, that the engine as well as the mill work, or manufacturing machinery it may drive, may be placed where most convenient, above or below the head or stream, to be worked by a fall of water from the least to the greatest height, or by any stream or river, the tail water below acting and having as much power as the head, answering to the height of either. Nor can a drop of water escape without performing its full duty. The power is therefore greater, and not liable to the disadvantages attending a water wheel. In tide waters it would work ebb and flood so long as there was a difference of two feet or less in the height, regulating itself, so that the power may be equal, let the head and tail water rise or fall, by which a smooth uniform motion is maintained and adapted to the smallest as well as the strongest power wanted. If the tail water re-acts upon a water wheel, it must lose so much of the power of the fall, or in other words, whenever the tail water rises above the lowest wash boards of the wheel, a counter action will be exerted as is well known against the power of the wheel. A considerable quantity of the stream must likewise escape without any good effect. These disadvantages of the water wheel, are removed by the syphon engine; and in frosty weather being fed by a pipe from below the ice, it will not be impeded, because its velocity in passing through the engine, will prevent the water being exposed sufficient time to the cold atmosphere to congeal it; and when at rest the engine may be left empty. It is scarce necessary to observe, that when the engine is fixed below the running head of the stream, it will fill as soon as the sluice is opened, and set itself to work; but when placed above the head, it will require sufficient water to fill the whole interior space, which being thus charged and converted into a syphon, will then work as well and with as much power as if the engine were placed below the head of the stream.

Other uses and applications of the engine.

Frosty weather affects it less than mills and other hydraulic machines.

I am,

Dear SIR,

your most obedient and

very humble servant,

JOHN HARRIOTT.

Thames Police, Dec. 20, 1802.

REMARKS.

## REMARKS.—W. N.

Consideration of  
the two principal  
objections to a  
water engine  
instead of a  
crane.

Labour of raising  
the Water,

compared with  
that now em-  
ployed to hoist  
and lower goods.

Men.

Horses.

Steam.

The labour  
saved by Mr.  
Harriott's engine  
(supplied by  
steam) would  
exceed six se-  
venths of that  
now expended.

Frost would  
most probably  
affect it in mill  
work as it does  
other engines;

The convenience and facilities afforded by Mr. Harriott's Engine, will be obvious to every one; and the only circumstances which can be urged in the way of objection against it, are those which he has himself noticed; namely, that it in general requires the water to be raised, which is to work it as a crane, and that this water is subject to freeze. To meet these difficulties fairly, we must admit, that if a ton of water were raised to the top of a warehouse, in order by its fall to raise a ton of goods (even if it could do that) it would be simpler for the warehouselman to raise his goods without the intervention of the fluid. But while we admit this general truth, it must also be observed, that the positions do not include the whole of the actual case. The raising and lowering of goods being a process of considerable skill and intelligence, and being necessarily carried on with many stops, interruptions, and variations of force, is on these accounts performed by the most expensive of all first movers; namely, the strength of men, and even this power is for a large part of the time inactive; namely, during all the intervals of operation. Here then is a wide field for the saving of force, if it could be stored up and used when wanted. Suppose for example, that two men were employed in a crane, and that the pauses of inactivity amounted to one third of their whole time, these men would be more beneficially employed in raising water, to be afterwards directed by a boy, or by the foot of a clerk who stood by, to keep account of the delivery of packages. But as horse work is reckoned at least five times as cheap as human labour, the saving of labour by employing that animal, would be about six sevenths of the whole; and steam engine work upon a very small scale indeed, would be as cheap again as horse work. Hence it appears, that after every allowance for the greater quantity of water required to produce velocity by its fall, and for other circumstances the saving of labour must be very great, by thus economizing and storing up the force, exclusive of the conveniences detailed by Mr. Harriott in his letter.

The freezing of the water is an impediment of such a nature, that it will perhaps be found that the operations of natural



tural streams or falls of water would be subject to interruptions from this cause, with his engine about as soon as with others that are more exposed, so that its advantages would consist of the other particulars which he has detailed. But with the engine applied for raising goods, it is to be apprehended that the water in the elevated reservoir would be bound up by freezing, and also in severe weather checked if not confined by the same cause in the engine itself. The remedy for this appears to be to work the engine, by raising the same mass of fluid repeatedly, and dissolving some cheap material in it which should render it less disposed to freeze. Most saline bodies would have this effect, and the quantity once added, would remain long without waste or loss. Experiment would shew what salt, and whether earthy, neutral, metallic, alkaline, or acid, might be the cheapest, most effectual, and least disposed to act upon the engine \*.

but in crane-work the water may be rendered incapable of freezing in our climate;

by a moderate addition of some cheap saline substance.

## IX.

*On the Electricity of the Shavings of Wood.*

*By Mr. W. WILSON.*

SIR,

*London, Dec. 13, 1802.*

YOUR readiness to insert my letter of the 11th of October, encourages me to request the insertion of the following account of experiments on the electricity obtained by cutting of wood, &c. In doing which you will very much oblige

your obedient humble servant,

WM. WILSON.

HAVING frequent occasions to work very dry wood that has lain over a large fire for several hours, I have often observed the shavings, &c. to adhere to the tools, and whatever they touched. About two years ago I began to take particular notice of, and endeavour to find the cause of this phe-

Wood shavings adhere to the tools.

\* In the consideration of this engine, the attention of the reader will be directed to the pressure engine of Mr. Trevithick, in our first volume.

nomenon, and after satisfying myself that it was not occasioned by any moisture or roughness on the substance; I suspected that electricity was the cause, and I accordingly set about the experiments which are the subject of this communication.

Dry warm beach  
shaved with glass  
is positive,

I laid a circular tin plate, 6 inches in diameter, on the cap of Bennet's electrometer, and with a piece of dry and warm window glass, scraped a piece of dry and warm beach, a few of the shavings being received on the tin plate, made the gold leaf strike the sides of the bottle with positive electricity. This was always the case whether the wood was hot or cold, but not always equally strong.

with steel nega-  
tive.

I thought a knife would be more convenient than glass to scrape with, and when I tried it, I found the shavings were negatively electrified, although they were taken from the same piece of wood which before gave positive.

Other woods.

This change in the result induced me to try different woods in different ways (sometimes scraping and sometimes cutting small chips) but obtained very uncertain results, for sometimes I obtained positive and sometimes negative electricity, even when I cut the same piece of wood with the same knife.

An insulated  
knife acquires  
the contrary  
electricity.

I next fixed the blade of a penknife into a glass tube, covered with sealing wax, and set to work with this insulated knife, and found that it was always electrified with the contrary electricity to that of the chips which were most frequently positive. But as they were sometimes negative, I repeated the experiments very often to discover the cause, but with very little satisfaction.

A sharp insulat-  
ed knife gave  
negative chips;  
a blunter posi-  
tive.

However, after making several hundred trials, I found the keenness of the edge of the knife had some influence, for one day after chipping with the insulated knife, and getting positive chips, I set the knife on a hone to make it cut better (which I had frequently done before) and when I began to chip the same piece of wood which but just before gave positive chips, I found the chips were negatively electrified several times, I then chipped the same piece of wood with a knife that had been very much used, without sharpening, and this gave positive chips as the other had done before it was sharpened; the knife that had been sharpened was tried again, but the chips were positive now, however when it was sharpened again it gave negative chips.

I now



I now thought I had found the cause of the contradictory results, but to be more certain about it, I began the following set of experiments. I sharpened a penknife to a very nice edge, and used the same pieces of wood that had been used in the former trials. In 24 trials with cherry-tree, the chips were always negative, and in four trials with elm, and in six with yew, the chips were always negative. I now drew the edge of the knife lightly over a piece of iron to dull it, expecting to get positive chips, but on trial the chips were still negative. Supposing the knife was not dull enough, I drew the edge over the iron again, and made the edge very bad, but the chips were still negative. The edge of the knife was next made rough by rubbing it on a grindstone, and this rough edge gave negative chips. The knife was next ground and set on the hone very carefully, and this sharp edge gave negative chips.

Other contradictory experiments.

As nothing satisfactory was obtained from this set of experiments. I began to suspect that the degree of heat of the wood was to be considered (for the wood was cold in all the last experiments, and sometimes hot, and sometimes cold, but most frequently hot in the first) or that perhaps the heat of the wood, and the sharpness of the knife, were both to be taken into the consideration. I therefore set about the following set of experiments.

Probability that the heat of the wood might influence the results.

I split the piece of cherry tree (that was used before) into two pieces, one of which was made thoroughly hot at the fire. This when chipped with the same knife that was used in the last experiments, without being sharpened, gave positive chips every time in six trials, and after this piece of wood had cooled till it was scarcely warm, gave positive chips every time in four trials. I then took the other piece which had not been near the fire for five or six hours, this gave negative chips every time in four trials. I now made this piece of wood quite hot, thoroughly, and chipped it again with the same knife, and in seven trials the chips were positive every time. These two pieces of wood was now laid by for three or four hours, to get quite cool. In this state they gave negative chips every time in twelve trials. One of them when made thoroughly hot again, gave positive chips every time in six trials. The other piece was now made warm (but only externally so) in eight trials it gave positive chips four times,

Detail of experiments with hot and cold wood; and different sharpness of the knife.



and negative chips four times; but after laying three or four hours to cool it gave negative chips every time in eight trials: A third piece of cherry-tree that had not been near a fire for four or five days gave negative chips. I repeated these experiments with different knives that had not very sharp edges, and with beach as well as cherry-tree. And whenever the wood was made thoroughly hot at the fire, it always gave positive chips, not only when hot, but when it was so cooled as not to be sensibly warm; but when it had laid away from the fire three or four hours, the chips were always negative. Sometimes when the wood was but slightly warmed, it would be very difficult to get any signs of electricity, and at other times when the wood was made hot only externally (by putting it very near the fire for a short time) the first few chips would be positive, and the succeeding ones negative. I had one instance where with the first chip the electrometer diverged near an inch, and with the second it completely closed again. Having succeeded thus far, I thought I would try whether the results would be the same if I used a very sharp knife, and accordingly sharpened two knives on a hone very carefully. And I used the same pieces of cherry-tree made thoroughly hot; in nine trials with one of the knives, the chips were negative every time and in five trials with the other knife, the chips were negative every time. I made a number of similar trials with a piece of beach with always the same results, but the beach seemed to be too hard for that keenness which is necessary to produce negative chips, for after cutting one or two chips, the edge would be spoiled, and produce positive chips; but always when the knife was sharpened, the first one or two chips would be negative. Similar to this I found by subsequent trials to be the case with the pieces of cherry-tree, ten or twelve chips of this (which was very straight and open grained) would spoil the edge.

General results. Shavings of dry wood by glass, are negative—hot wood by a moderate steel edge is positive, but negative if cold;—and a very sharp edge gives negative whether hot or cold.

From these experiments it appears, that when very dry wood is scraped with a piece of window glass, the shavings are always positively electrified. And if chipped with a knife, the chips are positively electrified if the wood is hot, the edge of the knife not very sharp, and negatively electrified if the wood is quite cold. But if the edge of the knife is very keen, the chips will be negatively electrified whether the wood is hot or cold.

The greatest number of trials was made with the insulated knife, which was always electrified contrarily to the chips; but the surface of the wood where the chips were cut from was very seldom electrified, and when it was it was always but weakly so, and of the same denomination as that of the weakest of the other two. I have repeatedly found that if a piece of dry and warm wood is suddenly split asunder, the two surfaces which were contiguous are electrified, one side positive and the other negative.

The knife always contrary.

Split wood has the two states.

## X.

*On the Composition of Emery.* By SMITHSON TENNANT,  
Esq. F. R. S. (*Ph. Transf.* 1802.)

THE substance called emery, which, from its great hardness, has been long used in various manufactures, for grinding and polishing other bodies, has not, it appears, been hitherto correctly analyzed. In books of mineralogy, it is considered as an ore of iron; an opinion probably derived from its great specific gravity, as well as from the iron which it frequently contains. But, where this metal is most abundant, it could not be extracted from it with advantage, and ought rather to be regarded as an impurity, as it does not contribute to produce the peculiar hardness for which this substance is distinguished. In Mr. Kirwan's mineralogy, he mentions an examination of emery made by Mr. Wiegleb, from which he inferred that 100 parts consisted of 95,6 of flint, and 4,4 of iron. Mr. Kirwan, however, justly suspects the correctness of this account, and observes that he had no doubt but some other stone was imposed on Mr. Wiegleb for emery.

Emery has not yet been correctly analyzed.

When powder of emery is boiled in acids, it becomes of a lighter colour, from the loss of part of the iron; after which, it does not seem to undergo any further alteration. As acids produce so little effect on it, I exposed it to a pretty strong red heat, with carbonate of soda, in a crucible of platina. On adding water to the mass contained in the crucible, the greater part of the emery was found in powder; having only become of a light colour, from the extraction of part of the iron.

Emery powder gives part of its iron to acids.

Soda by fusion dissolved some argil and leaves the emery.



iron. Though this process was twice repeated with the remaining powder, and in a stronger heat, a great porportion of it remained undissolved.

The alkaline solution, after a red calx of iron had subsided from it, was saturated with acid; and gave a precipitate of a white earth, which I found to be almost purely argillaceous.

This result is similar to Klaproth's with diamond spar.

The result of these experiments, was so similar to those of Mr. Klaproth on diamond spar, as to render it very probable that emery was in reality the same substance, though usually mixed with a larger proportion of iron; and the subsequent experiments appear to confirm this opinion.

Emery pulverized and cleared of magnetic particles.

In order to obtain a quantity of emery as free from iron as I could, I reduced to a coarse powder, a piece which consisted of different strata, some of which were of much lighter colour than others; and afterwards separated, by a magnet, the particles which were attracted by it. The part which was not attracted by the magnet, I observed to have the usual degree of hardness (by the scratches which might be made with it on flint.) I then reduced it to a finer powder, in an agate mortar; and, as this was principally done by pressure, and not by grinding, hardly any sensible addition was made to its weight. In the same manner, I found that diamond spar might be powdered to the same degree of fineness, without any material increase of weight from the mortar.

20 of the clear powder fused with 120 soda,

Of the emery powder thus prepared, 20 grains were taken, and heated in the manner before described, with 120 grains of soda, which had been previously deprived of carbonic acid, and boiled to dryness in a silver pan. By nearly the same process as that used by Mr. Klaproth, I obtained about 16,0 grains of argillaceous earth, 6 of siliceous earth, 8 or 9 of iron, and 6 of a grain remained undissolved. These numbers, reduced to parts of a hundred, are therefore,

and by the same process as Klaproth's the component parts of emery

Argillaceous earth	80
Silex	3
Iron	4
Undissolved	3

90.



Mr. Klaproth obtained from the Chinese corundum, after separating from it the particles which were attracted by the magnet, proved nearly the same as those of Chinese corundum.

Argillaceous earth	84
Silex	6,5
Iron	7,5
	<hr/>
	98.

As this analysis was no doubt conducted with greater care than mine, the loss of weight was less; but the proportion of the ingredients is sufficiently near to show that the substances are essentially the same.

From 25 grains of emery which appeared the most impregnated with iron, and yet retained its usual hardness, I obtained, argillaceous earth 12,5, silex 2, iron 8, and one grain was not dissolved; or, per cent.

Argillaceous earth	50
Silex	8
Iron	32
Undissolved	4
	<hr/>
	94.

As such emery can easily be had of uniform quality in large pieces, I procured the powder employed in this experiment, by rubbing two pieces against each other.

From 25 grains of emery, similar in appearance to the preceding, but which had been digested with marine acid previous to the action of the alkali, I had, Another analysis.

	per cent.
Of argillaceous earth	16,4 65,6
Siliceous earth	,8 3,2
Iron	2, 8,
Not dissolved	4,5 18,0
	<hr/>
	23,7 94,8.

The hardness of emery, as far as I could judge by its cutting rock crystal and flint, appeared to be equal to that of diamond spar. The latter could not be scratched by the former; but, as emery has not a surface sufficiently polished to render a mark visible, the converse of this could not be tried. Emery appears as hard as diamond spar.

Emery comes chiefly from Naxos.

It is cheap;

not crystallized;

is accompanied by the same substances as Chinese diamond spar.

All the emery which is used in England, is said to be brought from the Islands of the Archipelago, and principally from Naxos. In those places, it is probably very abundant; as the price of it in London, which I was told was 8 or 10 shillings the hundred weight, appears little more than sufficient for the charges of carriage. Though I saw a very large quantity in one place, (more than a thousand hundred weight,) I could not find any pieces of a crystallized form; possibly the great proportion of iron usually mixed with it, may prevent its crystallization. The whole consisted of angular blocks incrustated with iron ore, sometimes of an octaedral form, with pyrites, and very often with mica. The latter frequently penetrates the whole substance of the mass, giving it, when broken, a silvery appearance, if seen in the direction in which the flat surfaces present themselves to the eye. As these substances have no chemical relation to the emery itself, it is remarkable that they should also accompany the diamond spar from China; for Mr. Klaproth observes, "that its lateral facets are mostly coated with a firmly-adhering crust of micaceous scales, of a silvery lustre:" he also mentions, besides felspar, pyrites, and grains of magnetic iron ore.

## XI.

*Experiments and Observations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Essay on the same Subject. By JOHN DALTON\*.*

The properties of heat are continually under our notice.

Count Rumford's experiments on the transmission of heat by circulation through fluids.

THE nature and properties of fire or heat are subjects which present themselves to our consideration in almost every department of physics: it is no wonder therefore that new experiments, which point out and define the modes of operation of fire, before unobserved, or at least too much overlooked, should attract the attention of philosophers.—These observations were suggested upon reading Count Rumford's very ingenious experiments, in his essay above-mentioned, which exhibit a fact in a more striking point of view than it has ap-

\* Manchester Memoirs, vol. v. 373.

peared before—namely, *that the quickness of the circulation and diffusion of heat in fluids, is occasioned principally by the internal motion arising from a change of specific gravity affected by the heat.*—But the conclusion he has drawn from them—that *fluids are perfect non-conductors of heat*, in the way in which solids conduct it, appears to me totally unwarranted from the experiments, and erroneous in itself. And as it may be an error of practical consequence, if adopted, the exposition of it seemed desirable—which is the object of the following remarks and experiments.

His inference, that fluids are non-conductors is erroneous.

My first attempt was to ascertain the precise degree of cold at which water ceases to be further condensed—and likewise how much it expands in cooling below that degree to the temperature of freezing, or  $32^{\circ}$ . For this purpose I took a thermometer tube, such as would have given a scale of 10 inches with mercury from  $32^{\circ}$  to  $212^{\circ}$ , and filled it with pure water. I then graduated it by an accurate mercurial thermometer, putting them together in a basin filled with water of various degrees of heat, and stirring it occasionally: as it is well known, that water does not expand in proportion to its heat, it does not therefore afford a thermometric scale of equal parts, like quicksilver.

Experiments.

A thermometer made with water.

From repeated trials agreeing in the result, I find, that the water thermometer is at the lowest point of the scale it is capable of, that is, water is of the greatest density at  $42^{\circ}\frac{1}{2}$  of the mercurial thermometer. From  $41^{\circ}$  to  $44^{\circ}$  inclusively the variation is so small as to be just perceptible on the scale; but above or below those degrees, the expansion has an increasing ratio, and at  $32^{\circ}$  it amounts to  $\frac{1}{8}$ th of an inch, or about  $\frac{1}{160}$ th part of the whole expansion from  $42^{\circ}\frac{1}{2}$  to  $212^{\circ}$  or boiling heat.—During the investigation of this subject, my attention was arrested by the circumstance, that the expansion of water was the same for any number of degrees from the point of greatest condensation, no matter whether above or below it: thus I found that  $32^{\circ}$ , which are  $10^{\circ}\frac{1}{2}$  below the point of greatest density, agreed exactly with  $53^{\circ}$ , which are  $10^{\circ}\frac{1}{2}$  above the said point; and so did all the intermediate degrees on both sides. Consequently when the water thermometer stood at  $53^{\circ}$ , it was impossible to say, without a knowledge of other circumstances, whether its temperature was really  $53^{\circ}$ , or  $32^{\circ}$ . Recollecting some experiments of

Greatest contraction at  $42^{\circ}\frac{1}{2}$ .

Law of expansion.

It is the same on each side above and below  $42^{\circ}\frac{1}{2}$ .

Dr.



and the water  
may be cooled  
down as low as  
5° or 6° with-  
out freezing,  
&c. &c.

Dr. Blagden in the Philosophical Transactions, from which it appears that water was cooled down to 21° or 22° without freezing, I was curious to see how far this law of expansion would continue below the freezing point, previously to the congelation of the water, and therefore ventured to put the water thermometer into a mixture of snow and salt, about 25° below the freezing point, expecting the bulb to be burst when the sudden congelation took place. After taking it out of a mixture of snow and water, where it stood at 32° (that is 53° per scale) I immersed it into the cold mixture, when it rose, at first slowly, but increasing in velocity, it passed 60°, 70°, and was going up towards 80°, when I took it out to see if there was any ice in the bulb, but it remained perfectly transparent: I immersed it again, and raised it 75° per scale, when in an instant it darted up to 128°, and that moment taking it out, the bulb appeared white and opaque, the water within being frozen: fortunately it was not burst; and the liquid which was raised thus to the top of the scale was not thrown out, though the tube was unsealed. Upon applying the hand, the ice was melted, and the liquid resumed its station. This experiment was repeated and varied, at the expence of several thermometer bulbs, and it appeared that water may be cooled down in such circumstances, not only to 21°, but 5° or 6°, without freezing, and that the law of expansion abovementioned obtains in every part of the scale from 42°½ to 10° or below; so that the density of water at 10° is equal to the density at 75°. But as the discovery of this curious, and I believe hitherto unnoticed property, has little to do with the object before us, I shall say no more of it at present.

*(To be continued in our next.)*

## SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &amp;c.

*Lectureship on Subjects of Natural and Experimental Philosophy  
at Newcastle upon Tyne.*

AMONG our provincial societies for promoting the cultivation of literature and philosophical pursuits, the society at Newcastle upon Tyne, has for a number of years possessed an eminent station for the ability of its members, and the unremitting assiduity and intelligence which they have exerted to discover and carry into effect those objects which bodies of men are best capable of promoting. We all know that it is easy for a body of men to assemble together; to dignify themselves by a name; to hold forth striking pretensions to the uninstructed multitude; and to make a sort of commercial traffic of that scientific celebrity which the voices of men can confer upon each other, for a time at least, while the great public may ask in vain what they have done to deserve it. But when a body of true friends to their country and to mankind meet together, to consider in what way they can best employ their talents for the good of society and the promotion of the arts of civilized life, within that district which more immediately falls under the limit of their notice, and the extent of their power and influence;—when their well-earned celebrity is a secondary object, in comparison with the internal satisfaction afforded by a proper employ of talents and virtue—such an association becomes a public blessing, and its good consequences extend to the remotest periods of after times. The Literary and Philosophical Society of Newcastle, has already made inquiries into the state of the arts, of agriculture, and of the mineral products of their vicinity. It has assisted the diffusion of knowledge by the establishment of a well regulated and easily accessible library; and it has lately proceeded to constitute a lectureship on the subjects of natural and experimental philosophy, the requisite steps for which are now in progress with great spirit and activity.

Establishment of  
a philosophic  
lecture at New-  
castle upon  
Tyne.

Active exertions  
of the society at  
Newcastle.

On the fourth of May last, a paper by Mr. Thomas Bigge, History of the  
on the expediency of establishing a lectureship at Newcastle proposed lecture.  
upon Tyne, upon the subjects of natural and experimental  
philosophy, was read: In this paper, we find a clear, elegant Mr. Bigge's  
and proposal



adopted, and an  
address to the  
public circulated.

and animated statement of its objects; considered as well with regard to their great influence on society, as to those local circumstances towards which the intention of the author was more particularly directed. In this paper, no less striking for the value of its contents, than for the estimable motives of its author, we find facts which strongly support what has been said at the commencement of this article concerning the general proceedings of this respectable society. In consequence of this suggestion, the subject was again resumed, and an address to the public was circulated in the month of June, requesting their co-operation and assistance, exhibiting some part of the outline, and announcing that the appointment of Lecturer has been made to the Rev. Wm. Turner, a gentleman long and well known to the society for the abilities with which he had exercised the office of senior secretary. Since that time, considerable progress has been made in the subscriptions; and the lecturer delivered a general introductory discourse on Tuesday, November the 16th last, upon the objects, the advantages, and the intended plan of the lectures. I am sorry that the limits of a short notice forbid the attempt to give any analysis of this excellent discourse, which affords ample evidence of the comprehensive views of the author, with regard to the organization and duties of society in general, as well as the sciences he has undertaken to teach. I have thought it might be advantageous in some degree to the views of this public body to give the present account, short and imperfect as it must necessarily be, and still more to the world at large, if the exertions in favour of the sciences in one part of the kingdom, should, as is most probable, be followed by similar proceedings in others.

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*On a new Kind of Mortar called Plaster Cement.*

Account of a  
water cement.

Among the stones on the sea-beach, near Boulogne, a particular kind is collected, which when calcined and pounded like plaster, forms a very hard cement with water. This substance has been used for economical purposes, and was found to possess the valuable quality of resisting water; under which fluid, it hardens very strongly, and much more than in  
the



the air. Many constructions made with this cement afford the most complete proof of its solidity and tenacity. The detail of these experiments are given in a report presented by *Le Sage* to the society of Agriculture, Commerce and Arts, at Boulogne. These must no doubt be of much local importance; but the analysis of Guyton, as it tends to elucidate the composition of cements, is of more extended consequence.

The specific gravity was between 2,04 to 2,19 and 10 grammes, or about  $\frac{1}{3}$  of an ounce produced in centigrammes.

Lime	-	-	-	-	403
Carbonic acid	-	-	-	-	330
Clay	-	-	-	-	187
Oxide of iron	-	-	-	-	70
Alumine	-	-	-	-	5
					<hr/>
					995

The 187 centigrammes of clay afforded,

Silica	-	-	-	-	99
Alumine	-	-	-	-	39
Oxide of iron	-	-	-	-	43
					<hr/>
					181

The stones are therefore composed of

Lime	-	-	-	-	403
Carbonic acid	-	-	-	-	330
Oxide of iron	-	-	-	-	113
Silica	-	-	-	-	99
Alumine	-	-	-	-	44
Loss	-	-	-	-	11
					<hr/>
					1000

Citizen Guyton shewed a vessel to the society d'Encouragement at Paris, which was very close and firm in its texture, whence the abridger in the bulletin des Sciences, from which I take this article, expresses his opinion that it would prove very useful in the fabrication of various articles of pottery.

*Note concerning Two Brothers of a Race of Men resembling Porcupines.*

Men having  
scales and spines  
like porcupines.

Many philosophers have already spoken concerning this race, the reality of which has been established in a family well known by the name of Lambert. Two brothers of this family, all the males of which, have their bodies covered with spines or scales, are at present in Paris. One is 24 and the other 14 years of age. The body of the eldest is entirely covered, except the head and the inside of the hands and feet: the youngest is naked in some places, particularly about the breast; but the brown spots on those parts sufficiently indicate that when he advances in age, he will become as rough as his brother. The spines on the back of the hand are very large, and may be compared for their diameter to the quills of the porcupine; but those upon the breast have a greater resemblance to scales: they are small long plates, very numerous, and near together, being vertically implanted in the skin.

This thickening of the epidermis and hair, proceeds from a disease transmitted from generation to generation, but only from male to male. Five generations have been already afflicted with it.

*Bull. de la Soc. Philomath.*

Mawe's Mineral-  
ogy of Derby-  
shire.

*The Mineralogy of Derbyshire, with a Description of the most interesting Mines in the North of England, in Scotland, and in Wales; and an Analysis of Mr. Williams's Work, entitled, "The Mineral Kingdom." Subjoined is a Glossary of the Terms and Phrases used by Miners in Derbyshire. By JOHN MAWE, London. Phillips, George Yard, 1802, octavo, 211 Pages, with 4 Engravings.*

The author of this useful book was employed by a Spanish gentleman to make surveys of our principal mines in Derbyshire, to collect their various productions, and more particularly specimens from each stratum, describing their thickness, situation, and position; in order to shew an exact representation of the mines for the royal cabinet at Madrid. To the

observations thus made, he has added accounts of some mines in Scotland; a Tour from Glasgow to Staffa; the salt mines at Northwich; the Parys Mine; Observations in Wales, and an Account of Mr. Williams's book, in 2 vols. called the Mineral Kingdom. This account seems too concise to be of great utility, and as a critique would have answered its purpose as well, if a few of the epithets had been more civil.

*An Enquiry into the Causes of the Errors and Irregularities which take place in ascertaining the Strength of Spirituous Liquors, by the Hydrometer, with a Demonstration of the Practicability of simplifying and rendering this Instrument accurate. By WILLIAM SPEER, Supervisor and Assayer of Spirits in the Port of Dublin. Octavo, 48 Pages. London, 1802.*

On the present occasion, when the government of the country appears disposed to settle this point of experimental philosophy, upon which results of considerable importance to the revenue and to private property depend, but which have certainly been hitherto conducted in a slovenly and inaccurate manner, it must be very acceptable to all parties to see the subject clearly treated in a small pamphlet. After a short Introduction, the author gives an account of the origin and rise of the hydrometer, the causes of its irregularities, the different modes of charging overproof; the principal defects in Clarke's hydrometer; the imperfection of the weights in that instrument; the general principles of floating instruments; the standard of pure spirit; Gilpin's tables; modes of simplifying the hydrometer; description of his own improved hydrometer: with answers to objections that probably might be made to it; and he concludes with a chapter on the necessity of a standard for proof, and other practical objects of consequence in this business.

M. Speer's own hydrometer consists of a ball and stem, with counterpoise beneath as usual, and the upper or graduated stem is made of an octagonal form. Upon each of the eight faces is engraved a scale of per-centages; by inspection of which, the quality of the spirit is seen. But as the instrument will sink to different depths in the same spirit, according to

its

Description of Speer's hydrometer.—Stem octagonal; each side having the per-centages for different temperature.



Speer on the  
Hydrometer.

its temperature, the scale upon each of the faces is adapted to a determinate temperature, namely  $35^{\circ}$ ,  $40^{\circ}$ ,  $45^{\circ}$ , &c. till the last, which is for  $70^{\circ}$ . The temperature of the spirit being therefore known, the result must be read, upon that face, at the top of the which the known temperature is engraved; and to prevent any mistake, there is a small index to be put on the stem to direct the eye of the observer to the proper face. And moreover, as the temperature is shewn only to every five degrees, there is another index of a different colour, which performs the office of a weight, and shews the intermediate temperature. This may also be effected by warming the spirits, by holding the glass in the hand till its temperature agrees with that marked on one of the faces; or, as I understand from the specification of the author's patent, the precision of a single degree, if required, may also be obtained, by four small pins to be inserted, one for each intermediate degree in holes, in the counterpoise below, where in fact they operate as weights of adjustment.

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On the last day of this month, Mr. MACARTNEY will commence a COURSE OF LECTURES in the MEDICAL THEATRE OF SAINT BARTHOLOMEW'S HOSPITAL, on the *Anatomy of Animals and Vegetables*, from which will be deduced the general Doctrines of Physiology, or the laws of Organized Matter.

For Particulars, apply to Mr. NICHOLSON, at the Apothecary's Shop, St. Bartholomew's Hospital.

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A  
JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

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FEBRUARY, 1803.

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ARTICLE I.

*Answer to the Observations of Mr. WILLIAM CRUICKSHANK  
upon the Doctrine of Phlogiston\*. In a Letter from the Rev.  
JOSEPH PRIESTLEY, L. L. D. F. R. S. &c.*

To Mr. NICHOLSON.

*Northumberland, America, Nov. 5, 1802.*

S I R,

YESTERDAY only I received the fifth Number of your Introductory paragraph.  
valuable Journal, viz. for May, which contains Mr. Cruick-  
shank's reply to my defence of the doctrine of phlogiston in an-  
swer to him; and it gives me sincere pleasure to observe,  
that he shews so much zeal in the defence of his hypothesis,  
as by this means the truth will appear the sooner. I only la-  
ment, that my great distance will necessarily make the con-  
troversy of much longer continuance than it would otherwise  
be.

Mr. Cruickshank censures me for misquoting his words, and  
mistaking his meaning. If I have done so, I can assure him  
that it was without design; nor do I perceive that what he  
The quotation  
discussed by Mr.  
Cruikshank  
stated to be of  
no consequence  
to the argument.

\* Philosophical Journal, II. 48.

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F

alleges

alleges of this kind is of any consequence to the main argument. On the other hand, he says, p. 42, with an air of triumph, but before the victory, that "having additional difficulties to struggle with, I have been under the necessity of adopting new, and sometimes contradictory, opinions, in my explanations and defences."

The discussion attended with no difficulty.

Now I really am not sensible of any difficulty whatever in this discussion; and in advancing this he seems not to have apprehended my sentiments with respect to the calces of metals. If he was acquainted with my publications, he would have found that there was nothing either new, or contradictory, in what I have now advanced on this subject, or that of the composition of charcoal; though that I have sometimes changed my opinions on philosophical as well as other subjects, I readily acknowledge, and without any feeling of shame.

Finery cinder contains water only. For (upon adding phlogiston) water only is separated.

In my opinion, and that of long standing, the black calx of iron, commonly called *finery cinder*, contains no oxygen, but only water; and though the calx of zinc, and the yellow calx of lead, called *massicot*, do contain some, it is so little that I cannot detect it. Mr. Cruickshank says, p. 42, that "if water be the only substance contained in oxides, heat alone ought at least to revive some of them, and that in this case nothing but water would be separated." Now what I maintain is, that when finery cinder is revived, (which, however, is not done without the introduction of phlogiston) nothing but water is separated from it.

The present letter is confined to that object.

Oxygen is undoubtedly contained in the red calx of lead, called *minium*, and in that of mercury; but I say that even these cannot be revived without the introduction of phlogiston. This subject, however, I wave for the present, wishing to dispatch that of the air from finery cinder and charcoal before we proceed to any other; and I wish Mr. C. to attend to the following observations; which I shall now state more distinctly than I have done before, that it may be the easier for our readers to judge between us.

Mr. C's theory demands that oxygen should quit iron to form fixed air with charcoal, and then return to the iron in order

1. Mr. Cruickshank's hypothesis requires that, in the process of heating finery cinder and charcoal, the oxygen in the finery cinder should quit that substance, and unite with carbon in the charcoal, in order to form fixed air. Since, however, this fixed air is to be decomposed by iron, the oxygen which it has



has got from the finery cinder must be separated from it, and enter into the same calx again. But while the heat continues the same, I deem these contrary effects to be impossible. If the degree of heat that is applied expel oxygen from the calx, it will certainly prevent its return. Consequently, if fixed air be formed, it cannot be decomposed in these circumstances.

2. If it was possible for oxygen to be separated from finery cinder without any thing entering into it, which I think cannot be done, it could not, according to the new theory, form any thing by its union with carbon besides *fixed air*; this being said to be carbon dissolved in oxygen gas, but nothing inflammable could be produced by their union. Of this Mr. Cruickshank is sensible, and therefore he was under the necessity of supposing that, after the oxygen had quitted the finery cinder, it must enter into it again. But if this were possible, nothing would remain of the fixed air but *carbon*, as before that union; and this is a solid substance, incapable, without the aid of oxygen, of assuming the form of air. Whence, then, comes the inflammable air in this process, which so nearly resembles that from charcoal and water, that they must have the same origin; and with respect to *this*, Mr. Lavoisier decides, that whatever is inflammable in it must come from water.

3. Admitting all that Mr. Cruickshank alleges concerning the difference in specific gravity, and other circumstances, between the air from finery cinder and charcoal, and that from water and charcoal, it is not so great as the difference between the latter and the light inflammable air from the metals with acids or water. Different as they may be in other respects, they are all inflammable, and have the common property of uniting with oxygen in a certain degree of heat; in consequence of which they are the reverse of *oxides*, and must be classed with *combustible* substances, equally with sulphur and phosphorus.

4. If the oxygen, after quitting the finery cinder, entered into it again, it would make it finery cinder as at the first, or at least in some degree; whereas the calx is completely revived in this process, the iron so revived being as soluble in acids as any iron whatever.

5. If the iron should be completely revived by the oxygen wholly leaving it, I still maintain that it could not, by any

to leave oxide of carbon; which cannot happen at the same heat.

Carbon being considered as forming only one compound with oxygen (namely fixed air) it is concluded that no inflammable air could be had in reducing finery cinder; unless by means of water.

The inflammable gas, though different from that obtained by charcoal and water, is not an oxide but a combustible.

The oxygen does not return to the finery cinder; for the iron is reduced and continues so.

Iron cannot decompose fixed air. Mr. C's experiment left

decisive than  
that of the au-  
thor.

degree of heat decompose fixed air. For my experiment with a *burning lens*, in which it could not be done, is far more unexceptionable than Mr. Cruickshank's with *bladders and a gun barrel*. His objection to my process has no weight. It was made with a few ounce measures of the air over mercury, with a lens sixteen inches in diameter, and continued several hours, generally from ten o'clock to one; so that no particle of the air could escape being exposed to a far greater degree of heat than could be communicated through a gun barrel. His experiment I have frequently made both in England and here, but could never be satisfied with the result. The scale upon the iron, I have no doubt, came from moisture in the air, or from the bladders. Indeed, I cannot think that any person conversant as I have been with both these modes of operating, can hesitate in deciding that the preference must be given to mine.

Charcoal in-  
ferred to contain  
oxygen from Mr.  
Tenant's experi-  
ments.

6. Mr. Cruickshank seems to think that charcoal cannot contain any oxygen; but Mr. Tenant's fine experiment decisively proves that it does. For where are we to look for the oxygen (which we all acknowledge to be a component part of fixed air) which is separated from the marble, but in the charcoal which is produced, and in that it makes part of a solid substance, and does not take the form of air.

If oxygen existed  
in finery cinder  
it ought to pro-  
duce the phe-  
nomena of com-  
bustion in char-  
coal.

7. Since oxygen and all combustible substances unite, and explode, in a certain degree of heat, the oxygen that is expelled from the finery cinder uniting with carbon from the charcoal when red hot must enable it to *burn*; and therefore in these circumstances there ought to be an explosion, or at least a gradual combustion of them in the course of the process, as there is when oxygen is put to the same substance, and heated with it afterwards.

Invitation to  
Mr. Cruick-  
shank to enter  
into a full dis-  
cussion of the  
new theory.

It is now near twenty years since this new theory was advanced, and from that time to the present I have not ceased to express my opinion of its fallacy, and to give my reasons for that opinion; but I have not till very lately been able to draw any degree of attention to the subject. Now, however, I am happy to have succeeded in this; and as I find that the chemists in France, the great patrons of the system, look to Mr. Cruickshank as the ablest defender of it, I earnestly wish that he would undertake the discussion of every article of my objections to it. What he has animadverted upon is only *one*



out of *eleven* articles in my *Treat on Phlogiston*; and besides this, he should consider what I have advanced on the same subject in other publications, especially my experiments on the generation of air from water, both by evaporation and by freezing, and those on the pile of Volta, and also several articles in the Transactions of the Philosophical Society at Philadelphia.

As to the manner in which a controversy of this kind is conducted, whether it be expressive of *respect*, or *contempt*, it will have little weight with a judicious reader. Preferring however a plain and calm discussion,

I am,

Dear SIR,

Your's sincerely,

J. PRIESTLEY.

## II.

*The Construction of an Apparatus for conducting Sound and holding Conversations at a Distance. In a Letter from Mr. EZEKIEL WALKER.*

To Mr. NICHOLSON.

SIR,

SOME of the most important inventions were at first made use of, either as playthings for children, or in exhibitions to amuse the populace. Several very important inventions were at first considered as trifling.

Printing, which has contributed more to the improvement of the human mind than any other art, was invented for amusement, and the instruction of children\*. But the true value of an invention is not immediately seen, it has to go through a long series of improvements, before it arrives at a degree of perfection that is important to society. Printing.

The loadstone was long known to jugglers, and used by them in their exhibitions, before it was applied to the important purpose of navigation: and how far the improvements in Effects of the loadstone.

\* History of Holland by Adrian Young.



mechanics, optics, and acoustics, which are now exhibiting for amusement, may hereafter become useful in the common affairs of men, is a matter on which we are not at present able to judge.

Phantasms by the camera obscura behind a screen.

The daggers and death-heads which are made to appear in the air, by means of a concave mirror; and the ghosts and goblins, that are conjured up by means of the magic lantern of Philipsthal, not only serve as amusement, but they may contribute to check the growth of superstition, by shewing in an agreeable manner how easy it is to impose upon the senses.

Speaking machines.

Another way of deceiving the senses, is by speaking machines. These are of very ancient date, and have been so much improved by the moderns, as to attract much attention.

One by Thomas Irton in the 17th century.

No longer ago than the reign of Charles II. one Thomas Irton excited much wonder in the king and his whole court, by a machine of this kind \*. It is now well known that the sound was conveyed to the mouth of the statue by means of tubes artfully concealed; but the principle on which the speaking machines are now constructed, appears to be at this time very little understood.

Sound conducted through timber.

It was known in the days of Pliny, that if a long beam of timber received a slight stroke at one end, the sound was distinctly heard by a person whose ear was applied to the other end, though it could not be heard at the same distance through the air.

This property applied to a speaking apparatus.

From the following experiments which I made on this property of wood, it appears that acoustic instruments may be constructed for conversing at a distance, without the assistance of tubes to convey the sound.

### EXPERIMENT 1.

A deal rod 16 feet long was by contact made the medium between two trumpets.

I took a deal rod 16 feet long, and about an inch square, and after having fixed one end of it into the small end of a speaking trumpet, I laid it upon two supporters or props in an horizontal position. One of the props was placed under the trumpet, about three inches from its wide end, and the other prop was placed near the other end of the rod. Another speaking trumpet was then laid across the rod, about three inches from the end. The wide end of this trumpet

\* Beckmann's History of Inventions, III. 334.

rested upon the rod, but the other end was suspended by a ribband, from an object which need not be described.

After the apparatus had been prepared as above mentioned, I introduced my watch into the end trumpet, and on applying my ear to the cross trumpet, I heard the beats much louder than if the watch had been at the distance of a few inches only. The sound appeared to come out of the cross trumpet, although the watch was at the distance of seventeen feet and a half: and when the watch was laid into the cross trumpet, it was heard equally well at the end trumpet.

It may not be improper to mention, that the sound was increased by introducing a piece of metal, between the cross trumpet and the rod upon which it rested; but the manner of producing the best effect, I found by trial.

### EXPERIMENT 2.

I placed another prop under the middle of the rod, and laid several books upon it in different places; but the rod conveyed the sound just as well as before.

Bodies in contact with the rod did not prevent the effect.

### EXPERIMENT 3.

My assistant in these experiments being seated at the end trumpet, and myself at the other, a conversation took place through this apparatus; but in whispers too low to be heard through the air at that distance. The conversation afforded us much entertainment; for when the ear was placed in a certain position, the words were heard as if they had been spoken by an invisible being within the trumpet. And what rendered the deception still more pleasing, the words were more distinct, softer, and more musical, than if they had been spoken through the air.

Conversation held by sound passing through the rod both ways in a whisper.

### SPEAKING MACHINES.

What I have to say on this subject, must be so far anticipated by these experiments, as to render any particular description of mine unnecessary. It may not, however, be improper to mention my conjectures, respecting some useful purposes to which this acoustic instrument may be applied. If a communication founded on these principles, were made between a shop and the dining room, or warehouse, it might contribute to the dispatch of business: and instruments made

Useful purposes to which these machines may be applied.

on the same principles might, perhaps, be found convenient in private houses, if introduced between the parlour and some other room appropriated to the use of domestics. Directions might then be given to a servant without his entering the room, and in whispers too low to disturb the company.

I am,

SIR,

your's respectfully,

EZEKIEL WALKER.

*Lynn Regis, Jan. 19, 1803.*

### ERRATA.

Vol. IV. p. 40. in the first col. of the table, for 1, 2, 2, 4, read 1, 2, 3, 4.

P. 43. line 12, for long read large.

P. 43. line 19, for, In this manner, read Thus.

### III.

*Observations in Reply to Mr. GOUGH's Letter on the Grave Harmonics* \*. In a Letter from THOMAS YOUNG, M. D. F. R. S. &c.

To Mr. NICHOLSON.

SIR,

It is the opinion of Lagrange that the grave harmonics are not the results of imagination.

AT length Mr. Gough has accepted my invitation, and has adverted to the phenomena of the grave harmonics. These sounds, he thinks, are merely mental and imaginary; I suppose them real and material; Lagrange, whom I have already quoted, is of the same opinion; and while I have the authority of a man who is allowed to be either the first or second physical mathematician in Europe, I shall be very unwilling either to disbelieve my ears, or to confess that Mr. Gough has convicted me of error.

Development of the theory by that author.

To shew that I have not advanced a theory so new as Mr. Gough has deemed it, I shall quote a passage from the first volume of the *Miscellanea Taurinensia*. "We have seen," says Lagrange, "that the particle of air which is found in a

\* Philosophical Journal, IV. page 1. place



place where two sounds meet, receives an agitation different from that which is produced by each sound; if therefore the sounds are of such a nature that their vibrations coincide always after a certain given time, the continued and regular impression of these compound agitations may be distinguished from the simple agitations, and an ear sufficiently exercised will hear a third sound, of which the relation to the others may be found by comparing the number of separate vibrations that each of them completes between two successive coincidences," p. 103. "And the compound agitation may be conveyed to the ear in an infinite number of — situations." p. 104. "We have already spoken of the beats of Mr. Sauveur, and we have seen that they correspond exactly with the coincidences of the vibrations; there is therefore every reason to believe that they are formed in the same manner by the meeting of two sounds. And it is probable that the third sound of Tartini is only produced by a series of these beats." P. 105.

Were the contest to be decided by authority, it is probable that your readers would prefer that of Lagrange to Mr. Gough's and mine united: but we have no occasion for any thing more than reason and experiment. If the mind were capable of making up a sound in the way that Mr. Gough supposed, we ought to hear, whenever the impulses of one sound bisect either accurately or very nearly, the intervals between the impulses of another sound, an imaginary note, an octave above the separate sounds: if, on the contrary, my opinion is true, we must conclude that the retrograde motions of the one will counteract the direct motions of the other, and that both the sounds will be destroyed.

Happily the point thus at issue may be determined by a very simple experiment: we have two sounds standing in this relation, in the intervals between the beats of two musical chords tuned very nearly in unison. And if we listen to a grave sound which beats very slowly, while it is dying away, we shall observe, that in the interval of the last and faintest beats, when the sound is least mixed by reflections and irregular propagations, the note, instead of rising to the octave, is wholly lost.

I confess with pleasure, that Mr. Gough's explanation of the resemblance which I have pointed out between the perception of a faint sound from a tuning fork held be-

If the grave harmonics be the result of comparison; unisons, of which the vibrations bisect each other, ought to give the octave; — but if they result from coalescence of undulations, the sounds must here destroy each other.

Experiment to shew that the latter fact obtains in nature.

tween the teeth will be attended with a beat if another sound nearly unison be transmitted through the air : ception of a grave harmonic, and the sensation of a ringing in the ears, is ingenious and probable ; and I can mention a singular experiment in confirmation of his opinion. If a tuning fork, faintly vibrating, be held between the teeth, and a sound nearly approaching to the same note, be transmitted through the air, the beating will be nearly as distinct as if both sounds arrived through the same medium. From this circumstance, however, I only infer that all sounds enter the ultimate organ of hearing nearly enough in the same direction to produce an alternate intensification and remission.

The grave harmonics differ from primitive sounds ; but they no less real than compound tides, &c. I allow that such a sound differs from primitive sounds in its want of appropriate direction, and in its mode of propagation. The daily tide at Batsha in Tunquin neither comes from the east nor from the west, but it is as much a real tide as the grave harmonics are real sounds. If any person should insist that the phenomenon is not a tide, but an alternate elevation and depression of the water only, and that it exists only in the sensations of the observers, and not in the sea, I should be very little disposed to enter into arguments with him on the subject.

Remarks on the proper vehicles of philosophical papers. Philos. Transf.—Provincial Memoirs. Journals.

With respect to the communication of my remarks to the Manchester Society, I beg leave to reply, that if I thought a paper of permanent importance to the extension of science, I should consider myself as bound by my duty to the Royal Society and to posterity, to offer it for insertion in the Philosophical Transactions ; but if it were of a mere superficial and temporary nature, I should think it sufficient to publish it in a respectable literary Journal. I do not mean any disrespect to provincial societies, but many papers have appeared in the volumes of the Manchester Memoirs, which would perhaps have been more suitably placed in those of the Philosophical Transactions.

Your obedient,

humble servant,

THOMAS YOUNG.

Jan. 10, 1803.



## IV.

*Experiments and Observations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Essay on the same Subject. By JOHN DALTON.*

(Concluded from Page 58.)

COUNT Rumford's principal experiments are those in which a cake of ice was confined on the bottom of a cylindrical glass jar, of 4.7 inches in diameter, and 14 high, and water poured upon it of different temperatures suffering it to stand, without agitation. He found that about 6lb of boiling hot water melted little more ice than as much water of  $41^{\circ}$ ; and that by making such allowances as the experiments seemed to warrant for deductions when hot water was used, water of  $41^{\circ}$ , or  $9^{\circ}$  above the freezing point, melted quite as much, and often more, than the hot water: From which he infers, that water, and by analogy all other fluids, do not transmit heat in the manner that solids do, but circulate it solely by the internal motion of their particles.

Short recapitulation of Count Rumford's experiment of ice at the bottom of heated water.

The existence of this internal motion he has proved decidedly; that water of a certain temperature being of the greatest density, will always take the *lowest* place, and water either warmer or colder than that degree will ascend. This degree of greatest condensation he takes on the authority of others at  $40^{\circ}$ ; it appears however from the experiment related above, to be still more favourable to his position, namely  $42\frac{1}{2}^{\circ}$ : and that water of  $32^{\circ}$  must ascend till it comes to water of  $53^{\circ}$ , if it be not cooled in its progress, which circumstance he admits.

The internal motion or circulation.

Upon considering the facts related in his experiments therefore, there are *three* causes which suggest themselves as conspiring to circulate and diffuse the heat, by which the ice is melted.

Consideration of the facts:

1st. The internal motion of the liquid, by which water of  $32^{\circ}$ , incumbent upon the ice, is perpetually ascending into a warmer region of  $53^{\circ}$ , and warmer water of  $42\frac{1}{2}^{\circ}$  descending to take its place.

1. Cold water at  $32^{\circ}$  rises to the place of  $53^{\circ}$ , and warmer of  $42\frac{1}{2}^{\circ}$  descends.

2d. The proper conducting power of the liquid independent of internal motion.

2. The fluid itself may conduct.

3d. The



3. The glass itself may conduct; probably little.

Count Rumford does not allow that a cold fluid rising into a warmer can cool it;

whence it would follow that heat cannot be transmitted downwards throw water above  $42\frac{1}{2}^{\circ}$ .

He has not proved this by experiment.

3d. The conducting power of the glass jar. But as glass is known to be a very bad conductor of heat, it can produce no material effect in these experiments: for which reason Count Rumford does not appreciate the third cause.

With respect to the operation of the first cause, it will generally be supposed that cold water rising into warmer and remaining with it, the heat is impaired, and the two reduced to a common temperature. But Count Rumford does not admit of this communication; he maintains, that the two still retain their proper share of heat, notwithstanding they are mixed together. This hypothesis of his is of no peculiar consequence as far as respects the effect of the internal motion: for the temperature indicated by a thermometer immersed in an equal mixture of water at  $32^{\circ}$  and  $53^{\circ}$ , would be the same as if the water was uniformly of the temperature  $42\frac{1}{2}^{\circ}$ . But it has material consequences in other respects; for, if it be admitted, it annihilates the *second* cause abovementioned, and it would follow that warm water being put upon cold water above the temperature of  $42\frac{1}{2}^{\circ}$ , the heat could not in any degree be propagated downwards, unless by agitation, and even then, upon subsiding, the warm part ought to rise to the top, and the cold fall to the bottom.

These positions are so manifestly contradictory to common opinion, that they cannot be received without proof. But Count Rumford has not given us a single experiment to prove them. It seemed necessary therefore, to clear up this point by direct experiments.

#### Experiment 1.

Examination by Exper. 1. Hot water poured upon cold air in a glass vessel, the upper part gradually cooled, and the lower heated.

Took a large tumbler glass,  $3\frac{1}{2}$  inches diameter, and five inches deep, and filled it half way with water of  $51^{\circ}$ , then gently filled up the rest by means of a small syphon, with water of  $88^{\circ}$ ; a thermometer, with its bulb and stem detached from the frame, being previously immersed to the bottom. The temperatures at the top and middle were had by gently immersing the bulb of another thermometer into the water.

Time

# ON THE POWER OF FLUIDS TO CONDUCT HEAT.

Time elapsed.	TEMPERATURE		
	at top.	in the middle.	at bottom.
—	88°	—	51°
5 min.	85	—	54
12	83	75°	56
18	80	72 +	58
30	76	—	60
40	73	—	61
50	70	—	61
60	69	—	61

(Air in the room 50°.)

## Experiment 2.

The same as before ; only a circular piece of wood floated upon the surface of the water, on the centre of which the stream of the syphon was directed to prevent the current downwards.

Exp. 2. The same with more precaution.

(Air in the room 55°.)

TEMPERATURE.		
Time.	at top.	at bottom.
Before the water was poured on . . . . .		56°
—	116°	56 +
10 min.	105	56½
20	92	57 —
30	85	57⅓
40	80	57¾
50	77	58 +
1 h. —	75	58½
— 10	72½	58½
— 20	70	59 —
— 30	67	58½
— 40	65	58 +
— 50	63	58
2 —	62	58 —
— 15	61	57½
— 30	59½	57
3 —	57½	56 (Air 52°)
5 —	53½	53 Do.

A similar result was obtained in a different way by the following.

## Experiment

# ON THE POWER OF FLUIDS TO CONDUCT HEAT.

## Experiment 3.

3. The  
part of  
r heated by  
hot iron;  
lower part  
came also hot-

Took an ale glass of a conical figure,  $2\frac{1}{2}$  inches in diameter and 3 inches deep; filled it with water that had been standing in the room, and consequently of the temperature of the air nearly—Put the bulb of a thermometer to the bottom of the glass, the scale being out of the water: Then, having marked the temperature, I put the red hot tip of a poker, half an inch deep into the water, holding it there steadily about half a minute; and as soon as it was withdrawn, I dipt the bulb of a sensible thermometer into the water about  $\frac{1}{4}$  inch, when it rose in a few seconds to  $180^{\circ}$ .

## TEMPERATURE

Time.	at top.	middle.	bottom.
Before the poker was immersed . . . . .			$47^{\circ}$
—	$180^{\circ}$	—	47
5 min.	100	$60^{\circ}$	$47\frac{1}{2}$
20	70	60	49
1 h. —	55	—	52

These experiments all evidently agree in proving water to have a proper conducting power, independent of any internal motion. It surely will not be said that any slight motion unavoidably made at the beginning of an experiment, could continue with a powerful effect for upwards of an hour. However, to determine this matter, I made the two following experiments.

## Experiment 4.

Exp. 4. Co-  
loured water was  
heated by hot  
colourless water  
resting upon it,  
without mix-  
ture.

Took the glass tumbler of the first experiment, and filled it half way with rain water, deeply tinged with archil; then filled it up with clear warm water, as related in the 2d experiment. The upper half was but just perceptibly tinged by the process and uniformly so; it remained for an hour not visibly altered in this respect, though by frequently putting the bulb of a thermometer down to the middle, the colour at last rose in a small degree.



# ON THE POWER OF FLUIDS TO CONDUCT HEAT.

(Air 45°)

## TEMPERATURE

	<i>at top.</i>	<i>middle.</i>	<i>bottom.</i>
Before the warm water was poured on . . .			44°
<i>Time</i> . . .	105°	—	—
7 min.	97	—	47+
17	86	—	48
27	79	—	49+
37	75	68	50
47	70	66	50+
57	66	62	51½
1 h. 7	60	62	51½
— 17	60+	59	51½
— 27	59	—	51½

### Experiment 5.

A glass tube near an inch in diameter, and 16 inches long, was half filled with a coloured solution of common salt in water, warm; a small thermometer was wholly immersed in it, and cold clear water carefully poured upon the whole so as to fill the tube; the colour ascended very little, and continued invariable after the process of filling. The warm solution was of course made of greater specific gravity than the cold water.

(Air 45°)

## TEMPERATURE

<i>Time.</i>	<i>at top.</i>	<i>bottom.</i>
	45°	85°
5 min.	53	79
10	53	74
21	52	69
31	51	66
45	50½	64
58	50	61
1 h. 31	49	56½
3 30	47	51
— 55	47	50
4 15	—	49
7 5	46	48

To

# ON THE POWER OF FLUIDS TO CONDUCT HEAT

To determine whether hot and cold water being suddenly mixed, and agitated, the hot would afterwards rise to the top, was the object of

## Experiment 6.

6. Hot cold water being mixed, did not afterwards separate.

Air in the room  $50^{\circ}$ .—About  $\frac{1}{2}$  pint of water of  $130^{\circ}$  was poured into a cold tumbler glass, and immediately after as much water of  $50^{\circ}$ ; the mixture was agitated for half a minute by a deal rod; after which an immersed thermometer stood at  $85^{\circ}$ , both at top and bottom; it was then set by in a still place for examination.

## TEMPERATURE

Time.	at top.	bottom.
15 min.	$77\frac{1}{4}^{\circ}$	$77^{\circ}$
30	73	$72\frac{2}{3}$
45	68	$67\frac{2}{3}$
1 h. —	64.8	64.6

From all which water is concluded to be a proper conductor.

From all these experiments it is evident, that water has a proper conducting power: In the last experiment, if the particles of water during the agitation had not actually communicated their heat, the hot ones ought to have risen to the top, and the cold ones subsided so as to have made a material difference in the temperature. It is, however, equally evident, that water is a *bad conductor* of heat, probably as it is of electricity; the descent of the heat in the second experiment is wonderfully slow; a slight agitation for one *second* would do as much to induce the equilibrium as standing still one *hour*. In repeating the third experiment, in a wine glass, I have several times known water  $\frac{1}{2}$  an inch deeper to differ  $50^{\circ}$  in temperature from the incumbent water.

We must conclude, therefore, that the quick circulation of heat in water over a fire, &c. is owing *principally* to the internal motion excited by an alteration of specific gravity; but not *solely* to that cause as Count Rumford has inferred.

If it be proved that water conducts heat, it will scarcely be necessary to prove, that other fluids conduct it, and that they communicate it one to another:—The two following experiments shew that mercury conducts it, and that water and mercury reciprocally communicate it.

Experiment

*Experiment 7.*

Took a cylindrical glass tube, of 1 inch internal diameter, and put  $1\frac{1}{4}$  inches in depth of mercury into it, and immersed the bulb and stem of a thermometer to the bottom, the scale as usual being above the liquid; then put  $2\frac{1}{4}$  inches of warm water upon it by a syphon, and let it stand without agitation.

## TEMPERATURE.

<i>Time.</i>	<i>Merc.</i>	<i>Water.</i>
	56°	122
3 m.	70	118
6	73	110
11	75	100

## TEMPERATURE.

<i>Time.</i>	<i>Merc.</i>	<i>Water.</i>
14 m.	74 $\frac{1}{2}$ °	92°
19	73	87
27	71	78

*Experiment 8.*

Into a tumbler glass,  $2\frac{1}{2}$  inches in diameter, poured an inch in depth of mercury, and heated it to 110°; upon which was poured an inch of water at 50°, and then kept still.

## TEMPERATURE.

<i>Time.</i>	<i>Merc.</i>	<i>Water.</i>
	110°	50°
4 min.	74	70
8	71	70 $\frac{1}{2}$
10	70 $\frac{1}{2}$	70

Finding that water was so bad a conductor of heat, I was desirous to learn how ice would conduct it, and tried it as follows.

*Experiment 9.*

Feb. 9th. Out of a mass of ice, by means of a hot iron, I shaped a cylindrical piece, 3 inches in diameter, and  $5\frac{1}{2}$  inches long, clean and pure; its weight 17 ounces. Made a small round hole at one end, one inch deep, and the size of a thermometer bulb, which was inclosed in it. The other end of the piece was put into a basin of snow and salt, to the depth of from  $\frac{1}{2}$  to  $1\frac{1}{2}$  inches, the temperature of which was kept below 10° for  $1\frac{1}{4}$  hours. Air 37°.

	Therm. in the liquid.	Therm. in the ice.
Time elapsed.	5°	32°
$1\frac{1}{4}$ h. at a medium	7	31 $\frac{1}{2}$



N. B. This descent of half a degree was gradual, but did not commence till long after the beginning of the experiment. After this the piece of ice was inclined to one side, by which nearly one half of it was immersed in the cooling liquid, and the inclosed bulb of the thermometer was now not more than an inch from the cold mixture.

H.	M.	Therm. in the liquid.	Therm. in the ice.
1	50*	14°	28°
2	20	19	28
2	50	22	Ice along with the therm. slipped down into the cold liquid.

The ice now weighed  $12\frac{1}{2}$  ounces: the rest had been liquified by the operation of the saline liquor.

This experiment, I think, decidedly proves that ice is a worse conductor of heat than water: indeed this is not wonderful; for it is said, that ice at a low temperature becomes an electric.

Mixture is much more effectual for equalizing temperature, because hot and cold particles are brought together.

It is certainly a remarkable circumstance, but not at all inconsistent with the known laws of heat, that in a mixture of hot and cold liquids, the uniform temperature should be so soon induced by agitation and so slowly by rest: but when we consider, that in the former case, hot and cold particles are brought together, and that in the latter there is a series of particles one upon another, gradually rising in temperature, but differing by insensible degrees, we shall not wonder at the facts. When any one particle of water, or any other body, has one above it, warmer by an insensible degree, and another below it, colder by an insensible degree, its power to transmit heat must be very small†. These considerations gave rise to the two following experiments.

The gradual difference of temperature renders water a bad conductor.

#### Experiment 10.

Exp. 10. An heated mercurial thermometer exposed to air, gave out most heat when at the highest temperatures.

A mercurial thermometer was taken, its bulb  $\frac{4}{10}$  inch in diameter, and hanging clear of the scale: it was heated by the flame of a candle to 600°, and then laid upon a table with

\* From the beginning of the experiment.

† Will not this argument apply to solids universally? contrary to fact. N.

the

the bulb projecting over the edge, and was thus left to cool by the mere operation of the air in the room, which was  $52^{\circ}$ . The following is the medium result of two experiments, which, however, agreed with each other almost in every observation.

<i>Time.</i>	<i>Temp.</i>	<i>Time.</i>	<i>Temp.</i>
0	$600^{\circ}$	18 half m.	$66^{\circ}$
1 half m.	450	19	64
2	350	20	62
3	280	21	60
4	229	22	59
5	195	23	58
6	168	24	57
7	145	25	56
8	128	26	55
9	115	27	54
10	104	28	$53+$
11	95	29	53
12	88	30	53
13	81	31	53
14	77	32	$52+$
15	73	33	52
16	$69+$	Air in the room	52
17	68		

### Experiment 11.

Another thermometer, having a fimilar bulb, but a scale with much larger degrees, was heated and cooled in the same manner.

<i>Time.</i>	<i>Temp.</i>	<i>Time.</i>	<i>Temp.</i>
—	$85^{\circ}$	16	$56.3^{\circ}$
1 half m.	$79\frac{1}{2}$	17	$56\frac{1}{4}$
2	75	18	56
3	$71+$	19	55.9
4	$68\frac{1}{2}$	20	$55.7+$
5	$66\frac{1}{3}$	21	$55.6+$
6	$64\frac{1}{2}$	22	55.5
7	63	23	55.4
8	$61\frac{1}{2}$	24	55.3
9	60	25	55.25
10	59	26	$55.2-$
11	$58\frac{3}{4}$	27	$55.1+$
12	58	28	$55.1-$
13	$57\frac{1}{4}$	29	$55.+$
14	57	30	55
15	56.6		

In these experiments we may consider mercury and air mixed together of unequal temperatures, with a thin partition of glass—and from the last we may conclude, that the thermometer imparted to the air 40 times more heat in half a minute, when its temperature was  $30^{\circ}$  above the air, than when it was only  $1^{\circ}$  above it.

Argument from the Count's own experiments.

No water but that below  $53^{\circ}$  could descend to the ice, and consequently this only would be cooled, and the fusion of the ice would then cease.—Contrary to fact.

We shall now advert a little to Count Rumford's experiments. It will easily appear, that arguing fairly upon his own hypothesis he can never account for the phenomena observed: for, hot water being poured upon ice, an internal motion would take place near the surface of the ice, by which a stratum of water of a certain thickness would be reduced to  $32^{\circ}$ , and then all further reduction of the ice must cease; because all the superincumbent water being above  $53^{\circ}$  would be lighter and could not descend to the ice. But this is quite contrary to what took place. The facts, however, will admit of a satisfactory explanation upon established principles.

Explanation from the known facts. The ice is fused (1) by the proper conducting power of the water; and (2) by its motion. The first is as the heat, and the second diminishes in some unknown ratio as the temper. differs from  $42\frac{1}{2}^{\circ}$ .

Water at  $42\frac{1}{2}^{\circ}$  appears to fuse (chiefly by its motion) nearly the same as water at  $190^{\circ}$  (chiefly by its heat.)

By experiments 10 and 11, it appears, that the quantity of heat given out by a body, during any small given portion of time, is nearly as the excess of the temperature of the body above the cooling medium. Hence, then, we may conclude, that the effect of hot water upon ice arising from the proper conducting power of water, will be nearly as the heat of the water. What effect the other cause may produce, it will be difficult to determine from theory: experience will be the best guide. One thing, however, appears pretty certain, that its effect must be a *maximum*, when the temperature of the water at large is  $42\frac{1}{2}^{\circ}$ ; because then there can never want a determination of the particles *downward* to supply the place of the lighter water of  $32^{\circ}$  ascending. If the temperature of the water exceed  $42\frac{1}{2}^{\circ}$ , then the effect of the internal motion will be less, diminishing by some unknown ratio. As far as I can judge from Count R's experiments, the joint effects of those two causes should be nearly the same with water of  $42^{\circ}$  and water of  $190^{\circ}$ . Taking this, therefore, for granted, we shall be enabled to sketch a table of the values of these two causes for every  $10^{\circ}$  of temperature. The numbers expressing the effect of the proper conducting power, are derived from the 10th experiment, and consequently are not purely hypothetical: those expressing the other effect, except  $42^{\circ}$  and  $192^{\circ}$ , are put down hypothetically, because the law of decrease has not been ascertained.



It is to be supposed, that a given quantity of water, of the several temperatures mentioned, is carefully poured upon a cake of ice at the bottom of a cylindrical glass jar, and stands without agitation for a given time, as half an hour; then the proportionate quantity of ice supposed to be melted by the two causes separately are stated in numbers, and then the sums are taken to express the joint effects.

The results  
sketched out for  
different tem-  
peratures.

Temperature



After what has been said, I need not caution my readers not to consider this table as accurate. The principle of it, however, cannot I conceive be disproved: that the operation of the conducting power must be proportionate to a series of numbers beginning from 0 at  $32^{\circ}$ , and gradually increasing in some ratio with the temperature above  $32^{\circ}$ , cannot, I think, be controverted; and that the operation of the internal motion must begin from 0 at  $32^{\circ}$ , and increase till it arrives at its *maximum* at  $42\frac{1}{2}^{\circ}$ , and then decrease again ever after, is also, I apprehend, unquestionable: thus, when the jar had water of  $42^{\circ}$ , in Count R's experiments, this internal motion must have had a range of 8 inches in depth; whereas, when hot water alone was used, it had not more than  $\frac{3}{4}$  of an inch to range from the temperature of  $32$  to that of  $53^{\circ}$ .

The following table exhibits a concise view of all the material varieties of Count Rumford's experiments, with their results.

Table of the varieties of the Count's experiments.

Experiment



Experiment	Temperature of the water when poured on the ice.		Medium Tem- perature.	Water in the jar sur- rounded by		Ice melted in 30 minutes.
	At the beginning 41°	end 40°		Air	41°	
32			40½°	Air	41°	617 Grains
35	189	180	184½	{ a warm covering of cotton wool. }		747
38	41	43	42	Air	61	676
39	180	157	171½	Air	61	559
45	188	64	128	Ice & water		406
51	61	49	55	Ice & water		660
53	61	60	60½	Air	61	542

Count Rumford attempts to explain why there was less ice melted in such experiments as the 45th than in those like the 39th, and attributes the diminution of the effect to the descending currents, occasioned by the cold mixture surrounding the warm one, which he thinks would obstruct the opposite ones ascending from the ice. But the effect in the 51st, compared with the 53d, being just opposite, he passes over without explanation.—I have no doubt myself, but that the true cause of the differences in both cases, is to be found in the column expressing the *mean temperature* of the water, and not in that expressing its situation, which I consider as having nothing to do in the business, but as it affects the general temperature. The *maximum* effect with cold water will be when it is of the temperature of about  $48^{\circ}$  or  $50^{\circ}$ , and the *minimum* above it probably about  $100^{\circ}$  or  $120^{\circ}$ ; and in proportion as the mean temperatures, in any experiment, deviate from those points, the effects vary accordingly, let other circumstances be what they may.

Remarks.

The differences depend on the mean temperature of the water.

Thus I have attempted to explain the rationale of these very curious and interesting experiments, in a manner different to what their ingenious author has done. And must now leave it to the reader to form his opinion.

Conclusion.

## V.

*A Method of examining refractive and dispersive Powers, by prismatic Reflection.* By WILLIAM HYDE WOLLASTON, M. D.  
F. R. S. \*

IN examining the power with which various substances refract and disperse light, I have for some time past employed a method unnoticed by writers on optical subjects; and, as it is not only convenient in common cases of refraction, but also capable of affording results not attainable by other means, I have been induced to draw up a short account of the method itself, and of the most remarkable instances of its application.

New method of examining the refracting and dispersing powers of bodies, upon light;

This method was suggested by a consideration of Sir Isaac Newton's prismatic eye-glass, the principle of which depends on the reflection of light at the inner surface of a dense refracting medium.

Since the range of inclination within which total reflection

by angle of total reflection at the

\* Philof. Transf. 1802.

takes

confine of the denser of two mediums :

takes place, depends not only on the density of the reflecting prism, but also on the rarity of the medium adjacent to it, the extent of that range varies with the difference of the densities of the two media. When, therefore, the refractive power of one medium is known, that of any rarer medium may be learned, by examining at what angle a ray of light will be reflected from it.

General fact stated. The angle is greater, the greater the refractive density of the medium towards which the light passes ;

until total transmission.

For instance, when any object is laid under a prism of flint-glass, with air alone interposed, the internal angle of incidence at which the visual ray begins to be totally reflected, and at which the object ceases to be seen by refraction, is about  $39^{\circ} 10'$  ; but, when the object has been dipped in water, and brought into contact with the glass, it continues visible, by means of the higher refractive power of the water, as far as  $57\frac{1}{2}^{\circ}$  of incidence. When any kind of oil, or any resinous cement, is interposed, this angle is still greater, according to the refractive power of the medium employed ; and, by cements that refract more strongly than the glass, the object may be seen through the prism, at whatever angle of incidence it is viewed.

Fluids are easily applied to the dense medium (or glass) : But solids require the interposition of a fluid more refringent than themselves.

In examining the refractive powers of fluids, or of fusible substances, the requisite contact is easily obtained ; but, with solids, which can in few instances be made to touch to any great extent, this cannot be effected without the interposition of some fluid, or cement, of higher refractive power than the medium under examination. Since the surfaces of a stratum so interposed are parallel, it will not affect the total deviation of a ray passing through it, and may therefore be employed without risk of any error in consequence.

Thus, resin, or oil of saffras, interposed between plate glass and any other prism, will not alter the result.

A triangular glass prism may be used for comparing the powers of two bodies ;

If, on the same prism, a piece of selenite and another of plate-glass be cemented near each other, their powers may be compared with the same accuracy as if they were both in absolute contact with it.

but a square prism shews the sine of refraction without calculation :

For such a mere comparison of any two bodies, a common triangular prism is best adapted ; but, for the purpose of actual measurement of refractive powers, I have preferred the use of a square prism, because, with a very simple apparatus, it shows the sine of refractive power sought, without the need of any calculation.

Figure and application of the square prism.

Let A, Fig. 1, Plate IV. be a square or rectangular prism, to which



which any substance is applied at  $b$ , and let any ray of light parallel to  $cb$  be refracted through the prism, in the direction  $bde$ .

Then, if  $cf$  and  $ed$  be taken proportional to the sines that represent the refractive powers of the prism and of air,  $fg$ , which is intercepted between  $f$  and the perpendicular  $eg$ , will be the corresponding sine to represent the refractive power of the medium  $b$ . For, since  $edg$  (opposite to  $cf$ ) is the angle of refraction,  $efg$  (opposite to  $ed$ ) must be equal to the angle of incidence  $b dh$ ; and  $cf : fg :: bd : dh :: \text{fine of } cbi : \text{fine of } hbd$ .

All therefore that is requisite for determining the refractive power of  $h$ , is to find means of measuring the line  $fg$ . On this principle, the instrument in the annexed sketch (Fig. 2.) is constructed. On a board  $ab$  is fixed a piece of flat deal  $cd$ , to which, by a hinge at  $d$ , is jointed a second piece  $de$ , 10 inches long, carrying two plane sights at its extremities. At  $e$  is a second hinge, connecting  $ef$ , 15.83 inches long; and a third at the other extremity of  $ef$ , by which  $fg$  is connected with it. At  $i$  also is a hinge, uniting the radius  $ig$  to the middle of  $ef$ ; and then, since  $g$  moves in a semicircle  $egf$ , a line joining  $e$  and  $g$  would be perpendicular to  $fg$ .

The piece  $cd$  has a cavity in the middle of it, so that, when any substance is applied to the middle of the prism  $P$ , it may continue to rest horizontally on its extremities. When  $ed$  has been so elevated that the yellow rays in the fringe of colours (observable where perfect reflection terminates) are seen through the sights, the point  $g$ , by means of a vernier which it carries, shows by inspection the length of the sine of refraction sought.

The advantages which this method possesses above the usual mode of examining refractive powers, are greater than they may at first sight appear. The usual practice has been, to form two surfaces of the substance under examination, so inclined to each other that the deviation occasioned by them might be measured. The inclination of these surfaces to each other must also be known; and thence the refractive power might be computed. But, in the method here proposed, it is sufficient to have only one surface, and the result is obtained at once, without computation.

This method is most accurate and easy: it requires only one surface and gives the result at sight.

The facility of determining refractive powers, is consequently such as to render this property of bodies a very convenient test in many philosophical inquiries. For discovering the purity of essential oils, such an examination may be of considerable

Its facility renders it useful as a test in philosophical inquiries.

siderable utility, on account of the smallness of the quantity requisite for trial. In oil of cloves, for instance, I have met with a wide difference. The refractive power of genuine oil of cloves, is as high as 1,535; but I have also purchased oil by this name, which did not exceed 1,498, and which had probably been adulterated by some less refractive oil.

It is applicable to opake bodies.

For such purposes, the refractive power of opaque substances may often be deserving of inquiry, which could not be learned by any means at present in use. For, in the usual mode, a certain degree of transparency is absolutely necessary; but, for trial by contact, the most perfect opacity does not occasion the least impediment.

Instance.

Among other instances in which I have taken advantage of this circumstance, I may mention a substance that had been found in one of the islands of the North Pacific Ocean, which, to all outward appearance and by various trials, seemed to be perfect bees-wax, although it is supposed that there are no bees in the island from which it was brought. On placing it by the side of a piece of bees-wax, in contact with a prism, the perfect equality of their refractive powers afforded a strong confirmation of the opinion before formed of their identity.

Bodies of varying density may be examined by it,

For the examination also of media of which the refractive density is not uniform, the general method of trial by deviation wholly fails; on the contrary, by placing a varied medium in contact with a prism, all its gradations of density, from greatest to least, become at once the object of mere inspection. An instance of this may very readily be seen with a piece of gum, the surface of which has been moistened for a few minutes; when, by close application to a prism, a refractive power may be discerned, varying from that of the water on the surface, 1,336, to nearly 1,51, the refractive power of gum arabic.

particularly the crystalline lens.

I should not so much insist on this advantage, were it not for the opportunity hereby afforded of examining the crystalline lens of the eye, which is now known to be generally more dense in the centre than at its surface.

Mr. Hawksbee, who was not aware of this difference, has estimated the refractive power of the crystalline lens, by forming it into a wedge by plates of glass, somewhat higher than I find it to be; but, with his accustomed accuracy, he remarked the apparent enlargement of an object, occasioned by the variations of its density, which he was unable to explain.

In the table that follows, I have set down, not only the limits of



of refractive power in a crystalline lens of an ox, ascertained by trial, but also an average, computed from the refractive density of a dried crystalline of an ox, of which the weight had been first taken in the recent state, and the quantity of water lost by drying also measured.

The table exhibits a series of substances, arranged according to their refractive powers. That of the diamond is copied from Sir Isaac-Newton; of other bodies to which (on account of their being more dense than glass) the machine for measurement would not apply, the refractive powers have been found by other means, for the sake of furnishing a more continued series of subjects for comparative experiments. The rest have been compared by this method; and their power, when expressed in numbers, actually measured.

Table of refractive powers.

TABLE I.

Diamond	2,44	Oil of saffrafas	1,536
Plumbago	—	Red sealing-wax	—
Native sulphur (double)	2,04	Spermaceti, cold	—
Glass, consisting of lead	—	Sugar, after fusion	—
6 and sand 1	1,987	Arsenate of potash	—
Glass of antimony	1,98	Mastic	—
Jargon	1,95	Elemi	—
Spinnelle ruby	1,812	White wax (cold)	—
Arsenic	1,811	Oil of cloves	1,535
Muriate of antimony, variable	—	Copal	1,535
White sapphire	1,768	Anise	1,535
Gum dragon	—	Radcliffe crown glass	1,533
Iceland spar, strongest	1,657	Pitch	—
Sulphate of barytes (double)	1,646	Centre of crystalline of fish, and dry crystalline of an ox	1,530
Balsam of Tolu	1,60	Canada balsam	1,528
Guaiacum	1,596	Crown glass, common	1,525
Benzoin	—	Selenite	1,525
Flint glass	1,586	Caoutchouc	1,524
Ditto	1,583	Gum lac	—
Horn	—	Dutch plate glass	1,517
Phosphorus	1,579	Human cuticle	—
Mica	—	Gum arabic	1,514
Opium	—	Balsam of capivi	1,507
Amber	1,547	Oil of amber	1,505
Rock crystal (double)	1,547	English plate glass	1,504
Old plate glass	1,545	French plate glass	1,500
Colophony	1,543	Oil of nutmeg	1,497
Box-wood	—	Sulphate of potash	1,495
Bees-wax	1,542	Tallow	—



Tallow, cold	-	1,49	Cryſtalline lens of an ox	1,447
Iceland ſpar, weakeſt		1,488	to	- 1,380
Camphor	-	1,487	Computed average of	
Linſeed oil	-	1,485	ditto	- 1,430
Butter, cold	-	1,480	Sulphuric acid	- 1,435
Eſſence of lemon	-	1,476	Fluor ſpar	- 1,433
Oil of turpentine, com-			Nitric acid (ſp. gr. 1,48)	1,410
mon	-	1,476	Alcohol	- 1,37
————— rectified		1,470	White of an egg	- 1,36
Oil of almonds		—	Æther	- 1,358
— olives	-	1,469	Vitreous humour of an	
— peppermint	-	1,468	eye	- 1,336
— lavender	-	1,467	Water	- 1,336
Tallow, melted	-	1,460	Atmoſpheric air	
Alum	-	1,457	(Haukſbee)	- 1,00032
Spermaceſti, melted	-	1,446		

## ON THE DISPERSION OF LIGHT.

This method is  
likewiſe appli-  
cable to ſhew the  
diſperſion of  
light.

The limit of  
perfect reflec-  
tion is marked  
by a fringe of  
colours;

except when  
(the refractions  
being unequal)  
the diſperſions  
are equal.

If the diſperſion  
by the rarer me-  
dium be greateſt  
the order of  
colours will be  
inverted.

The method above deſcribed for investigating refractive powers, may alſo be employed with ſimilar advantage for inquiries into the diſperſion of light by different bodies, and the conſequences that reſult from their combined action.

When a glaſs priſm is placed in contact with water, and brought near the eye, in ſuch a poſition that it reflects the light from a window, the extent of perfect reflection is ſeen to be bounded by a fringe of the prismatic colours, in the order of their refrangibility.\* The violent rays, being in this caſe the moſt refrangible, appear ſtrongeſt and loweſt, on account of the leſs obliquity that is requiſite for their reflection.

But it may happen that two media, which refract unequally at the ſame incidence, may diſperſe equally at that incidence. Under theſe circumſtances, a pencil of rays paſſing from one of ſuch media into the other, will be refracted, without diſperſion of its colours. The boundary of prismatic reflection would then be found a well defined line, free from colour, if the ſurface at which the reflected light emerges from the priſm, were at right angles to its courſe.

When the diſparity of the diſperſive powers of the media is ſtill greater, it may alſo happen, that the uſual order of prismatic colours will be reverſed; and then the red will appear ſtrongeſt and loweſt in the fringe, unleſs the colours ſo pro-

\* Newton's Optics. Book i. part 2. Exp. 16.

duced are counteracted by-refraction at their emergence from the prism.

An instance in which the colours are so reversed, may be seen by application of oil of saffras to a prism of flint glass. Instance: F. glass and oil of saffras.

So high is the dispersive power of this oil, that, in refractions from flint glass into it, the red rays are refracted more than the violet.

It must be observed that, in this experiment, when the angle of reflection within a triangular prism exceeds  $60^\circ$ , the angle of emergence is such as would alone occasion the red rays to appear lowermost; but, when the glass used is rectangular, the refraction at emergence has an opposite effect; any reversion of colour will therefore be in some degree corrected, and may not be seen, unless the dispersive power of the medium in contact much exceeds that of the glass.

A case of refraction with an inverted order of colours, has been observed by Dr. Blair,\* in a compound object-glass where crown-glass was in contact with oil of turpentine. From trials with lenses, he likewise inferred, that several other fluids have the same effect, when applied to that glass. Crown glass and oil of turpentine.

With this glass, and also with plate-glass, I have tried oil of turpentine, and many other fluids that afford a similar reversion of colours, as linseed-oil, olive-oil, the essential oils of bergamot, lemon, lavender, pennyroyal, and peppermint, strong nitric acid, and many artificial compounds that I shall presently have occasion to mention. other oils, &c.

The dispersive power of fluor spar is the least of any substance yet examined; so that, although its refractive power is also remarkably low, (considering its great specific gravity,) a prism of fluor, in contact with water or alcohol, shows the prismatic colours to be refracted in an inverted order. Fluor spar disperses very little.

With heavy spar, the instances of reversion are very numerous, as its dispersive power is low, and is accompanied with great refractive density. In the refractions from this spar into flint glass, and into all oils or resins, I believe, without exception, the colours are seen reversed. also ponderous spar

Rock crystal likewise disperses so little, that it exhibits the colours reversed, when it is in contact with many substances of less refractive power than itself. I have tried it with Dutch plate-glass, with Canada balsam and balsam of capivi, with and rock crystals

\* Edinb. Transf. Vol. III.

many oils essential and expressed, and have found the colours in all these cases reversed.

Metallic salts  
disperse much.

By solutions of metallic salts, a great variety of such appearances may be produced. Most of these compounds have a highly dispersive power; and many of them may be rendered sufficiently dense to occasion reversion, even when applied to flint-glasses. In a more dilute state, they may be used with crown-glasses, or plate-glasses, to produce the same effect. And since, when further diluted by a less dispersive medium, they will also present an appearance of colourless refraction, we may, by examining the degree of dilution necessary for that purpose, compare the dispersive powers of any ingredients contained in them, and may gradually extend our knowledge of this property to the elements of any bodies, however compounded.

Experiments of  
diluting them  
till no colour  
appeared.

As a specimen of the method, I have in this way compared a few solutions of metals, and of other substances, that were each diluted till the limit of reflection appeared void of colour, when they were in contact with a rectangular piece of plate-glass; and, in the table which follows, I have expressed their refractive powers in that state of dilution, as nearly as the eye can discern the disappearance of colour.

TABLE II.

Table of their densities in that state.		In Water.		In Alcohol.	
		1,364		1,390	
	Nitro-muriate of gold	-	-	-	-
	Nitro-muriate of platina	-	-	-	-
	Nitrate of iron	-	-	-	-
	Sulphuret of potash	-	-	-	-
	Red muriate of iron	-	-	-	-
	Nitrate of magnesia	-	-	-	-
	Nitric acid	-	-	-	-
	Nitrate of jargon	-	-	-	-
	Balsam of Tolu	-	-	-	1,400
	Acite of litharge (extract of lead)	-	-	-	-
	Nitrate of silver	-	-	-	-
	Nitrate of copper	-	-	-	-
	Oil of sassafras	-	-	-	1,405
	Muriate of antimony	-	-	-	1,410
	Nitrate of lime	-	-	-	1,422
	Nitrate of zinc	-	-	-	-
	Green muriate of iron	-	-	-	1,415
	Muriate of magnesia	-	-	-	1,416
	— of lime	-	-	-	1,425
	— of zinc	-	-	-	1,425
	Essence of lemon	-	-	-	1,430
	Balsam of capivi	-	-	-	1,440



It may here be seen, that several of the metals increase the Gold and platina  
dispersive powers of nitric and muriatic acids, and consequently most increase the  
exceed them in that respect. Of all these substances that dispersion; zinc  
have yet tried, gold and platina are the most dispersive. The least.  
least dispersive of the metals is zinc.

The earths also are found to possess this property in very The earths differ  
different degrees: that of the jargon and magnesia differ but much.  
little from nitric acid in dispersive power; but siliceous earth,  
on the contrary, is inferior to water.

By comparing the salts formed with the nitric and muriatic Nitric acid dis-  
acids, it appeared probable that the former had the higher dif- perses more than  
persive power; but a more direct comparison could not be made muriatic.  
by means of the rectangular piece of plate-glass, as muriatic  
acid could not be rendered sufficiently dense for such a trial; I  
therefore made use of a triangular prism of crown-glass, which  
is in itself less dispersive than any plate-glass, and, from the  
relative position of its surfaces, occasioned less correction of the  
colours. With this prism, I found that strong muriatic acid  
(having a refractive power 1,394) exhibited the colours re-  
versed; and that, when it was diluted till the limit of reflection  
appeared void of colour, its refractive power was reduced to  
1,382. But the dispersive power of nitric acid, when tried  
by the same prism, proved to be greater; for this acid required  
to be diluted till its refractive power did not exceed 1,375,  
before the colour was wholly destroyed.

In the table it may be observed, that the red and green mu- More oxygen  
riates of iron, though consisting of the same metal and acid, seems to produce  
differ very much in dispersive power; and, consequently, that more dispersion  
some caution will be necessary, in attempting to compare the in the muriate  
of iron;  
different metals with each other by means of the salts contain-  
ing them, as any difference observed may be owing in part to  
a difference in the quantity of acid to which they are united,  
and in part to their different proportion of oxygen.

A striking instance of the latter is manifest, from a compa- but less in the  
rison of sulphur with the sulphuric acid; for, while the former oxidation of  
appears to exceed the metallic oxides in dispersive power, the sulphur.  
latter is inferior even to water.

As I have likewise, at various times, made many experi- The order of  
ments on dispersion by means of wedges, in a manner nearly various bodies as  
similar to that employed by Mr. Dollond, Dr. Blair, and others, to their dis-  
persive powers;

I have endeavoured to reduce the several substances thus examined to one table; but, as the limits of colour are in few instances sufficiently well defined for accurate mensuration, I have not attempted to add any numerical estimate of their powers, but have merely ascertained the order in which they succeed each other; and, in the following table, have arranged them according to the excess of their effect on violet above red light, at a given angle of deviation.

TABLE III.

tabulated with their refractive powers annexed.	Order of dispersive Powers.	Refr. Power.	Order of dispersive Powers.	Refr. Power.
	Sulphur - - -	2,04	Amber - - -	1,547
	Glass of lead ( $\frac{1}{4}$ sand) -	1,987	Diamond - - -	2,44
	Balsam of Tolu - -	1,60	Alum - - -	1,457
	Oil of saffraas - -	1,536	Plate-glass, Dutch -	1,517
	Muriate of antimony -		Ditto, English - -	1,504
	Guaiacum - - -	1,596	Crown glass - - -	1,533
	Oil of cloves - - -	1,535	Ruby (spinelle) - -	1,812
	Flint-glass - - -	1,586	Water - - -	1,336
	Colophony - - -	1,543	Sulphuric acid - -	1,435
	Canada balsam - -	1,528	Alcohol - - -	1,37
	Oil of amber - - -	1,505	Sulphate of barytes -	1,646
	Jargon - - -	1,95	Selenite - - -	1,525
	Oil of turpentine -	1,47	Rock crystal - - -	1,547
	Copal - - -	1,535	Sulphate of potash -	1,495
	Balsam of capivi -	1,507	White sapphire - -	1,768
	Anime - - -	1,535	Fluor spar - - -	1,433
	Iceland spar - - -	1,657		

Numerous  
achromatic com-  
binations.

By comparison of this table with the order of refractive powers, as contained in the first table, it will be seen how little correspondence there is between them; and, accordingly, how numerous are the combinations by means of which a pencil of rays that passes through two media, may be made to deviate without dispersion of its colours.

White light does  
not appear to be  
separable by the  
prism into seven  
nor into three  
colours.

I cannot conclude these observations on dispersion, without remarking that the colours into which a beam of white light is separable by refraction, appear to me to be neither 7, as they usually are seen in the rainbow, nor reducible by any means (that I can find) to 3, as some persons have conceived; but that, by employing a very narrow pencil of light, 4 primary divisions of the prismatic spectrum may be seen, with a degree of distinctness that, I believe, has not been described nor observed before.

If a beam of day-light be admitted into a dark room by a crevice  $\frac{1}{8}$  of an inch broad, and received by the eye at the distance of 10 or 12 feet, through a prism of flint-glass, *free from veins*, held near the eye, the beam is seen to be separated into the four following colours only, red, yellowish green, blue, and violet; in the proportions represented in Fig. 3.

The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet. But C, the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark lines, *f* and *g*, either of which, in an imperfect experiment, might be mistaken for the boundary of these colours.

The position of the prism in which the colours are most clearly divided, is when the incident light makes about equal angles with two of its sides. I then found that the spaces AB, BC, CD, DE, occupied by them, were nearly as the numbers 16, 23, 36, 25.

Since the proportions of these colours to each other have been supposed by Dr. Blair to vary according to the medium by which they are produced, I have compared with this appearance, the coloured images caused by prismatic vessels containing substances supposed by him to differ most in this respect, such as strong but colourless nitric acid, rectified oil of turpentine, very pale oil of saffras, and Canada balsam, also nearly colourless. With each of these, I have found the same arrangement of these four colours, and, in similar positions of the prisms, as nearly as I could judge, the same proportions of them.

But, when the inclination of any prism is altered so as to increase the dispersion of the colours, the proportions of them to each other are then also changed; so that the spaces AC and CE, instead of being as before 39 and 61, may be found altered as far as 42 and 58.\*

H 2

By

\* Although what I have above described comprises the whole of the prismatic spectrum that can be rendered visible, there also pass on each side of it other rays, whereof the eye is not sensible. From Dr. Herschel's experiments (Phil. Trans. for 1800) we learn, that

on



Curious effects  
of the refraction  
of candle light;

and the light of  
electricity.

and disoxy-  
genating rays  
on the confine  
of violet.

By candle-light, a different set of appearances may be distinguished. When a very narrow line of the blue light at the lower part of the flame is examined alone, in the same manner, through a prism, the spectrum, instead of appearing a series of lights of different hues contiguous, may be seen divided into five images, at a distance from each other. The 1st is broad red, terminated by a bright line of yellow; the 2d and 3d are both green; the 4th and 5th are blue, the last of which appears to correspond with the division of blue and violet in the solar spectrum, or the line D of Fig. 3.

When the object viewed is a blue line of electric light, I have found the spectrum to be also separated into several images; but the phenomena are somewhat different from the preceding. It is, however, needless to describe minutely, appearances which vary according to the brilliancy of the light, and which I cannot undertake to explain.

on one side there are invisible rays occasioning heat, that are less refrangible than red light; and on the other I have myself observed, (and the same remark has been made by Mr. Ritter,) that there are likewise invisible rays of another kind, that are more refracted than the violet. It is by their chemical effects alone that the existence of these can be discovered; and, by far the most delicate test of their presence is the white muriate of silver.

To Scheele, among many valuable discoveries, we are indebted for having first duly distinguished between radiant heat and light; (*Traité de l'Air et du Feu*, § 56, 57;) and to him also we owe the observation, that when muriate of silver is exposed to the common prismatic spectrum, it is blackened more in the violet than in any other kind of light. (§ 66.) In repeating this experiment, I found that the blackness extended not only through the space occupied by the violet, but to an equal degree, and to about an equal distance, beyond the visible spectrum; and that, by narrowing the pencil of light received on the prism, the discoloration may be made to fall almost entirely beyond the violet.

It would appear therefore, that this and other effects usually attributed to light, are not in fact owing to any of the rays usually perceived, but to invisible rays that accompany them; and that, if we include two kinds that are invisible, we may distinguish, upon the whole, six species of rays into which a sun-beam is divisible by refraction.

## VI.

*An Account of Dr. YOUNG's Harmonic Sliders. From his Paper in the Journals of the Royal Institution, p. 261.*

THE combination of undulations, however cautiously the world may adopt its application to the explanation of optical phenomena, is of acknowledged utility in illustrating the phenomena of musical consonances and dissonances, and of undeni-  
Utility of the doctrine of combined undulations in explaining cotemporary sounds; and the tides.  
 able importance in accounting for many of the phenomena of the tides. Each tide is an undulation on a large scale; and, supposing the general form of the ocean, in consequence of the attraction of a distant body, to coincide with that of an oblong spheroid, as it is found by calculation to do, the section of the surface of each tide, if conceived to be unbent from the circular form and extended on a plane, would form the harmonic curve. (Young's Syllabus, IV. 151. 155.) It is remarkable that the motions of the particles of the air in sound have been generally supposed in theory to correspond with the ordinates of this same curve, and that there is also experimental reason to believe, that the purest and most homogeneous sounds do in fact agree very nearly with the law of this curve. It is therefore by far the most natural as well as the most convenient to be assumed, as representing the state of an undulation in general; and the name of these harmonic sliders is very properly de-  
The harmonic sliders  
 duced from the harmonic curve.

By means of this instrument, the process of nature, in the combinations of motion which take place in various cases of the junction of undulations, is rendered visible and intelligible, with great ease, in the most complicated cases. It is unnecessary to explain here, how accurately both the situations and motions of the particles of air, in sound, may be represented by the ordinates of the curve at different points: it is sufficient to consider them as merely indicating the height of the water constituting a tide, or a wave of any kind, which exists at once in its whole extent, and of which each point passes also in succession through any given place of observation. We have then to examine what will be the effect of two tides, produced by different causes, when united. In order to represent this effect, we must add to the elevations or depressions in consequence of the first tide, the elevations or de-  
visibly exhibit the process of nature in the junction of undulations.  
Explanation applied to tides.  
 pressions

pressions in consequence of the second, and subtract them when they counteract the effect of the first: or we may add the whole height of the second above any given point or line, and then subtract, from all the sums, the distance of the point assumed below the medium.

Exhibition of a simple tide,

and of combinations which either increase, or modify, or destroy each other.

The principle is applicable to a variety of cases,

undulations differing in extent.

The beat of an imperfect unison.

To do this mechanically is the object of the harmonic sliders. The surface of the first tide is represented by the curvilinear termination of a single board, Plate VI. Fig. 1. The second tide is also represented by the termination of another surface; but, in order that the height at each point may be added to the height of the first tide, the surface is cut transversely into a number of separate pieces or sliders, which are confined within a groove or frame, and tightened by a screw, Fig. 2. Their lower ends are situated originally in a right line; but, by loosening the screw and moving the sliders, they may be made to assume any other form: thus they may be applied to the surface representing the first tide; and if the similar parts of each correspond, Fig. 3, the combination will represent a tide of twice the magnitude of the simple tides.

The more the corresponding parts are separated, the weaker will be the joint effect, Fig. 4; and, when they are furthest removed, the whole tides, if equal, will be annihilated, Fig. 5. Thus, when the general tide of the ocean arrives by two different channels at the same port, at such intervals of time that the high water of one would happen at the same instant with the low water of the other, the whole effect is destroyed, except so far as the partial tides differ in magnitude. The principle being once understood, it may easily be applied to a multiplicity of cases: for instance, where the undulations differ in their dimensions with regard to extent. Thus, the series of sliders being extended to three or four alternations, the effect of combining undulations in the ratio of 2 to 1 or 3 to 1, of 2 to 3, of 3 to 4, may be ascertained, by making a fixed surface, terminating in a series of curves, that bear to those of the sliding surface the ratio required: and, by making them differ but slightly, the phenomenon of the beating of an imperfect unison in music may be imitated, where the joint undulation becomes alternately redoubled and evanescent. In Fig. 6, the proportion is that of 17 to 18, and the curvilinear outline represents the progress of the joint sound from the greatest degree of intensity to the least, and a little beyond it.

VII. *Observations*



## VII.

*Observations on the Appearances produced by the Collision of Steel with Hard Bodies. By Mr. DAVY, Lecturer on Chemistry at the Royal Institution\*.*

I. MR. HAWKSBEЕ long ago shewed, that no sparks could be produced by the collision of flint and steel† in the exhausted receiver of an air pump, and that in this case a faint light only was perceived. And, since his time, the same observation has been very often made.

Hawksbee's exp. of flint and steel in vacuo.

The developement of the theory of combustion has clearly shown that the vivid sparks obtained from steel in the atmosphere, are owing to the combination of the small abraded and heated metallic particles with oxygen. But it has been a matter of doubt whether, in the experiment made in vacuo, the faint, luminous appearance is owing wholly to the light produced by the fracture and abrasion of the parts of the flint, or only partly to this cause, and partly to the ignition of the minute filaments separated from the steel.

Sparks thus produced in air, are owing to combustion of the steel.

II. I have often found, that when a fine and thin flint, which may be easily broken, is used for the collision in vacuo, the light is much more vivid than when a thick and strong one is employed: and with a strong flint, but just sharp enough to give sparks with steel in the atmosphere, it is seldom that any light at all is produced in the exhausted receiver. These facts seem to shew that the abraded particles of steel are not rendered at all luminous by collision, except in consequence of combustion; and the opinion is almost fully proved by the following experiment, which was made in the course of a lecture on the properties of light, in the theatre of the Royal Institution, and which has been since often repeated.

Light in vacuo is most vivid with a thin sharp flint.

The collision does not make the steel red hot.

III. A thin piece of iron pyrites‡ (sulphuret of iron) was inserted in a gunlock in the place of the flint. It gave by

Pyrites instead of flint, gave no light in vacuo.

\* Journal of the R. I. 264.

† Philosophical Transactions, Vol. XXIV. p. 2165.

‡ The etymology of the name of this substance shews that its property of giving fire by collision was very anciently known. It was used in the old gunlocks, with the revolving wheel, for inflaming the priming.

collision in the atmosphere very vivid sparks; which were chiefly white, from the combustion of the particles of the steel; but sometimes mixed with a few red sparks from the combustion of the particles of the pyrites. The gunlock was introduced under the receiver of an air pump, and the exhaustion was made till the mercury in the short gage stood at about  $\frac{6}{10}$  of an inch. The lock was then snapped, but no light whatever was perceived; and the phenomenon was uniform, every precaution being taken to render the room dark, and to preserve the apparatus in order.

Question. Can steel burn in air if not first white hot?

If it cannot, why is not this white heat seen in vacuo?

IV. It is well known that in common cases the finest steel wire does not burn with a white light or sparks in the atmosphere, unless it have been previously heated to a degree much above that of the red heat; it consequently at first view appears extraordinary, that the particles separated from the gunlock should be heated so as to burn vividly in air, and yet not so as to appear ignited in vacuo; for it is not easy to conceive that they emit light, which from the minuteness of their volume\* cannot be perceived; or to suppose that the opacity of the metallic substances should hinder light generated at their points of contact from being visible. I had formerly supposed, in reasoning upon the phenomenon of the collision of flint and steel, that † heat and light might in common instances be only accidentally coexistent; and that in certain cases very high temperatures might be produced without causing the appearance of light. At present however I am inclined to believe, that the phenomena may be adequately accounted for upon principles that coincide with the common facts relating to the production and communication of heat.

Explanation. Oxidation at temperatures far below ignition, may develop more heat than can be conducted off, and may produce vivid combustion;

Mr. Stodart ‡ has shewn, that when steel is gradually heated it begins to change colour at about 430° Fahrenheit. And this change of colour is occasioned by its combination with oxygen, and, as there is every reason to believe, must be connected with the evolution of heat. At about 600°, a temperature much below that of ignition, it oxidates rapidly, and becomes covered with a bluish grey coating. And though in these cases of oxidation the heat evolved at the surface of

\* Or the short time of its emission.—N.

† Nicholson's Journal, 4to, Vol. III. p. 517.

‡ Nicholson's Journal, 4to, Vol. IV. p. 130.



the metal is not sufficient to raise the temperature of steel wire, or a steel plate, so as to cause it to enter into the vivid combustion, yet in acting upon such a minute filament as that struck off in the gunlock, it may be sufficient to keep up the process of oxidation till it becomes so vivid as to occasion the strongest heat and light. Besides, the surface of this filament is very great as compared with its bulk, and the oxide produced upon it is less likely to form a coat which might defend the interior parts from the action of the air \*.

It would not be difficult to find many analogous instances, in which the progress of oxidation is dependent upon the mass of the combustible body, or rather upon the relation of this mass to surface; thus, a very thin and small bit of phosphorus will inflame spontaneously, and burn with the vivid light when wrapped up in filaments of fine cotton; whilst a thicker and larger piece will only shine with the feeble blue light: and though a large mass of zinc may be melted in the atmosphere without inflaming, a small and thin shaving will burn vividly long before it is heated to the temperature of fusion.

V. In considering the general phenomena of the production of heat and light, by mechanical means, it is difficult to conceive that any considerable increase of temperature can be produced on a metallic surface by a single collision; for the conducting power of the metals is such as would speedily cause the heat to be communicated to the contiguous parts; and even in the case of the abrasion of minute particles, though the time required for their separation from the mass is to us imperceptible, yet it must be sufficient to enable them to give out to it a portion of their heat.

The bodies that become luminous by being struck or rubbed together in vacuo, or in gases that do not contain oxygen, or under water, such as fluat and carbonate of lime, siliceous stones, glass, sugar, and many of the compound salts, are both electrics *per se*, and phosphorescent substances; so that the flashes they produce are most likely occasioned, partly by

in case the surface be very great in proportion to the mass.

Other instances. Phosphorus and zinc readily take fire in small masses.

Not probable that much heat should be produced by a single collision.

Luminous appearances, from electricity or phosphorescence, by friction.

\* In turning very fine work of steel in the lathe, so as to afford shavings or threads much thinner than one-thousandth of an inch, I found that this metallic wool very readily caught fire at a candle, and burned throughout in quantities of a cubic inch or more: But it was scarcely so much oxidized as to have lost its flexibility after this combustion.—N.

the



Cases of actual  
ignition:

the electricity excited on their surfaces by the friction, and partly by their phosphorescence, which is generally occasioned by moderate degrees of heat. It is not however improbable that in some cases, by the collision of very hard stony bodies, which are bad conductors of heat, there may be an actual ignition of abraded particles; and the supposition is countenanced by various facts. Mr. T. Wedgwood found that a piece of window glass, when brought in contact with a revolving wheel of grit, became red hot at its point of friction, and gave off luminous particles which were capable of inflaming gunpowder and hydrogen gas \*. And we are informed by a late voyager †, that the natives of Oonalashka light their fires by striking together two pieces of quartz, their surfaces being previously rubbed with native sulphur, over dry grafs.

### VIII.

*Description of a Blow-pipe by Alcohol, having a safety Valve, with other Advantages. Constructed by Mr. BENJAMIN HOOKE, Fleet Street.*

To Mr. NICHOLSON.

SIR,

Blow-pipe with  
a safety valve and  
only one flame.

I HAVE taken the liberty of troubling you with a drawing and description of the *blow-pipe by alcohol* as I make it, which perhaps possesses the following advantages over that described in your number of September last; viz. Being furnished with a safety valve, to prevent accidents; having only one lamp (the wick of which being pretty large, answers both for heating the alcohol and for affording a strong blast when drawn through it); and I think superiority as to form and appearance.

Should you esteem it worthy a place in your valuable Journal, the insertion will oblige,

SIR,

Your very humble servant,

159, Fleet Street,  
Nov. 20, 1802.

BENJAMIN HOOKE.

\* Phil. Transf. 1792, p. 45.

† Sauer's Account of Billings's Expedition to the northern parts of Russia, p. 159.

A is a hollow sphere \* for containing alcohol, resting upon a shoulder in the ring O, Plate V. Description.

B is a bent tube with a jet at the end, to convey the alcohol in the state of vapour into the flame at Q, this tube is continued in the inside up to C, which admits of A being filled nearly, without any alcohol running over.

D is a safety valve, the pressure of which is determined at pleasure, by screwing higher or lower on the pillar E, the two milled nuts F and G carrying the steel arm H, which rests on the valve.

I is an opening for putting in the alcohol.

K is the lamp, which adjusts to different distances from A, by sliding up or down the two pillars L. The distance of the flame Q from the jet, is regulated by the pipe which holds the wick being a little removed from the centre of the brass piece M, and of course revolving in a circle.

N the mahogany stand.

## IX.

*Description of a Joint applied to Tubes used for conveying Steam under considerable Pressure. W. N.*

THE valuable communication of my correspondent N. N. in the last number, has fixed my recollection upon several parts of my hydraulic and pneumatic apparatus, in which, though they may be called bagatelles as far as relates to the magnitude of the inventive powers required to make them, yet their utility may be such as in many instances to prevent less eligible contrivances from being adopted. Recollection of some apparatus.

In the first place, I find among some brass work the ingenious joint, Plate II. Fig. 2, lately contrived by himself, and before him by Mr. Webster, but in this case certainly made many years ago by some unknown mechanic. My piece, which is for spouting fluids, is prevented from being driven asunder by a wire that passes through its axis, and has a small nut and washer faced with leather. Early invention of the joint for pneumatic apparatus.

\* If the bottom is made flat instead of spherical, the action of the flame will then be greater.

The

Another joint  
for strong steam.

The apparatus of which I have given a drawing and section in Plate VII. was made for conveying steam from a boiler to the steam engine, and for many other philosophical purposes, in which this agent is very useful, though hitherto not much applied to objects of this nature. As I have found it very commodious, I should not be disposed to make any remarks on the contrivance itself, if its resemblance to so many things that I have seen, both in organization and effect, did not make it proper to say, that I should have let it pass unpublished (among the many arrangements, whether old or new, which every practical mechanic finds himself induced to adopt in his operations) had not the consideration of utility made me suppose it might prove acceptable.

Delineation. A  
wide conical  
cock with joints  
in the tubes,  
pressed together  
with a screw and  
a dish spring.

In Fig. 1, 2, and 3, the same letters of the alphabet denote the same things; Fig. 1 exhibiting a view, Fig. 2 a section, and Fig. 3 the plan of some of the parts. The letter *e* shews part of an upright tube conveying steam from the boiler into the body of the cock *c* by its hollow cone *m*, which is open beneath and closed above, and has a side aperture *n* communicating, when duly placed, with the pipe *h* *i*. The cone is pressed into its place by the cap *b* screwed on the body of the cock *c* at *r*. The cap does not press immediately upon the upper surface or shoulder of the cone, because the motion would in that case be hard and uncertain; but it acts by the intervention of a spring *o* in the shape of a dish or slightly concave place, which keeps it to its bearing with any required degree of pressure accordingly as the cap is more or less screwed down, and always with a most smooth and pleasant action without the least jerk or obstacle. The cock is turned by the wooden handle or lever *a*, and is very secure on account of the large surface of the convex and concave cones which remains imperforated.

The pipe between *h* and *i* is connected by a joint, in which *g* is a loose cap milled on the outside, and having a concave screw on its inner surface. It is screwed upon the fixed piece *f*, which is turned to fit it: In this last piece is a cylindric concavity surrounding the orifice of the pipe *h* (see *f*, Fig. 3), into which the piece *p*, at the extremity of the pipe *i*, loosely fits. Upon the circular face of *i* are turned three or four grooves, and the like on the correspondent face of *f*. These are of use to secure a small piece of tallowed linen cloth put between them



them when applied together. Lastly, there is a dish spring *o* between *p* and *g*. When *g* (which slides loosely upon the pipe *i*) is screwed upon *f*, it presses *p* by the intervention of the stiff dish spring against the interior of *f*, and makes a secure junction; but the pipe *i*, represented in Fig. 1 as pendant, or in a vertical position, may be moved round in the same plane to any elevation or depression whatever.

The cap *d* being screwed on the body of the cock at *t*, constitutes a joint of the same kind, by means of which the pipe *i* may be moved into any horizontal position: Consequently, by these two motions, that pipe may be placed in every possible situation.

If we suppose the pipe *i* that proceeds immediately from the cock, to be of considerable length, as for convenience it is required to be, its possible changes of position will not be sufficient to adapt it to the usual operative purposes. It may therefore, in several cases, be convenient to apply two other similar joints *k* and *l* at right angles with each other, by means of which it will be immediately seen, that the exterior pipe *w* may be placed in any required direction, not merely with regard to a point immediately above the boiler, but with regard to another moveable point at an equal distance from the fire.

I shall conclude this paper by observing, that the bended washer or dish spring is used in the vice and some other mechanical tools, but that it far from being as much used as it ought to be. For there is scarcely any action of pinching, pressing, or binding, in which it would not be advantageous; and would, in many cases, prevent serious mischief and inconveniences.

The dish spring generally recommended.

## X.

*Memoir on Achromatic Glasses adapted to the Measure of Angles, and the Advantages that may be derived from double Refraction for the precise Measurement of small Angles: by ALEXIS ROCHON, Member of the National Institute, and Director of the Naval Observatory at Brest\*.*

The position of places determined by angles.

Euler said to have first attempted to correct the aberrations of refrangibility by a lens of glass and water;

and afterward Dollond by flint glass and crown glass;

an invention which he is said to have stolen from Hall.

WHEN we would determine the position of places, either on the earth or in the heavens, our conclusions are always founded on angles obtained by observation; and of course the certainty of our conclusions must depend on the precision with which we can measure these angles †. Euler was the first who attempted to correct the aberrations of refrangibility, by employing substances of different refractive powers. Maupertius procured Euler's object glass, consisting of glass and water, to be made at Paris: but this we now know could not succeed, from the ratios the refractions and dispersions of common glass and water bear to each other.

In 1755, Mr. Dollond laid before the public his achromatic object glasses, composed of flint glass and crown glass. He says in his paper in the Philosophical Transactions, that he destroyed the aberrations of refrangibility with tolerable ease; but to destroy that of sphericity at the same time, was an object much more difficult to surmount. For this Dollond obtained a patent, the exclusive privilege of which was attacked by Mr. Watkins of Charing Cross, on the ground that the discovery had been made long before by a gentleman of the name of Hall, who however had not rendered it public. It is said that Mr. Hall gave orders for his lenses of flint glass to an optician in one part of London, and for those of crown glass to another in a distant part, that his secret might not be discovered; and that both of them happened to employ the same workman to make them, who suspecting some mystery, as he

\* Abridged from the Journal de Physique, Frustrator, year 9.—C.

† The history of the invention of achromatic glasses is given very erroneously in this and most foreign memoirs. I purpose soon to give a commentary on this subject.—N.

knew

knew they were for the same person, shewed them to Dollond, for whom likewise he worked, and who thus learned what he afterwards published as his own discovery.

Be it as it may, I shall with difficulty be persuaded, that Euler was not the first who thought of correcting the aberrations of refrangibility, as Newton invented the means of correcting the aberrations of sphericity, by composing an object glass of glass and water. Was it not the illustrious Euler, who, reflecting on the structure of the eye, suspected this organ was composed of different mediums for the purpose of destroying the confusion produced by the decomposition of light when it traverses a single medium? And did he not publish in the Memoirs of Berlin and of Petersburg, that sublime idea, which accorded with the system of perfection he ascribed to all the works of the Supreme? It is true, the inutility of this correction of the aberrations for the sphere of distinct vision, from the shortness of the form of the eye, may be objected to Euler's hypothesis: but this makes nothing against his claim to the discovery, to which he was led by it. Neither had this happy idea that success at the time, which might have been hoped from it; partly because the theory of this great geometrician was founded on laws of refraction purely hypothetical; partly because it was repugnant to the assertion of Newton, that, when light traverses several mediums of different natures, so that the emergent rays are parallel to the incident, the light is not decomposed. It is well known, that the learned Klingenshiern, in 1755, expressed his doubts of the laws of refraction advanced by Newton: and that these doubts were confirmed in 1759, by Dollond's experiments on crown and flint glass, which first exhibited to the learned colourless prisms.

It was in the beginning of 1774, that I read to the academy a memoir, in which I proposed to improve achromatic object glasses by the interposition of a fluid between the two lenses of crown and flint glass which compose them. Borda, le Gentil, and Cassini, were appointed a committee to repeat my experiments. They observe in their report, that, "in an object glass of three lenses, if there be a difference of a thousandth part of a line between the curvature of the centre and that of the edges of each surface, a sensible imperfection in the image of the object will be the consequence. But the mere heat of the workman's hand, in giving the glass the finishing polish, is capable of dilating

But Euler the first discoverer of the principle,

to which he was led from reflecting on the structure of the eye.

Though the correction he ascribed to the structure of the eye not necessary in this organ.

His idea neglected, because his theory was hypothetical, and contradictory to an assertion of Newton's.

Klingenshiern expressed his doubts of Newton's laws of refraction; and Dollond confirmed these doubts.

Rochon proposes to improve Dollond's compounded object glass, by introducing a fluid between the pieces.

Report of Borda and others on this subject.



Their experiments on it, which are in favour of the principle.

lating it sensibly, so that the difficulty of not producing some inequalities of curvature in large glasses must be very great. Perhaps it is impossible to avoid this imperfection, but it may be corrected: and our colleague affirms, that the effect of the imperfections of the four interior surfaces of the three glasses may be diminished considerably, by the introduction of a diaphanous fluid between them. Experiments alone can decide the fact. We took therefore an achromatic telescope three feet long, the aperture of which was about three inches. The two lenses composing the object glass, being about half an inch distant from each other, we introduced between them a thin Bohemian glass not wrought. In this state it is obvious the telescope must be very bad; and accordingly, on directing it to some writing, we were obliged to bring this within eleven yards and half of it, to distinguish the letters. Having then poured pure water between the object glasses, so as to fill the intervals between them, we found the letters not more difficult to be distinguished at the distance of sixty-two yards. We might have pursued our experiments further, but this was sufficient to establish the principle, which was all we had in view."

To make a perfect object glass, the refractive power of the interposed fluid should be the same with that of the glass.

Water reduces the confusion to one seventh;

but oil, though its power is nearer that of glass, does not reduce it so much.

At this time I was ordered to Brest, which put a stop to my pursuit. In my memoir I had advanced, that the confusion arising from the irregularity of the glass of a telescope, was to the confusion which takes place when the glass is immersed in a fluid, as the refraction of the glass is to the difference of refraction between the glass and the fluid: consequently, if the fluid have the same refractive power as the glass, no alteration in the distinctness of the object will be perceptible; but if the glass be immersed in water, the confusion will be about a seventh only of what would have taken place, had the object been seen directly through the irregular glass. This calculation, it is true, supposes the glass to be in perfect contact with the fluid; but there are obviously many causes by which this contact may be influenced. It may have been owing to some such cause, that oil, the refractive power of which differs less from that of glass, was found by experiment not to answer so well as water; for writing, which was legible at the distance of 130 feet when water was employed, could be read only at eighty-eight feet when oil was interposed between the glasses\*.

\* On this subject, see *Philos. Journal*, 4to, III. 090.

By the Registers of the Academy of Sciences, January the 6th, 1788, it appears, that Grateloup adopted the same method. He did not make use of a fluid interposed between the glasses, however, but of mastic. This substance had long been in use among jewellers, for cementing together stones, which it did in such a way, that it was scarcely possible to distinguish the two so united from a single stone; and hence Grateloup conceived, it would render his object glasses as it were one solid piece, without being liable to evaporation like a fluid, while it corrected the errors of their interior surfaces. Glasses cemented together with mastic, however, do not answer, at least for sea voyages, as change of temperature, and the sea air, affect the mastic very much. On this account I have preferred resin, and even turpentine the most fluid and transparent I could get. Chemists perhaps may find substances still preferable to these, and it is an object certainly worthy their attention. Dr. Blair tried a great number of solutions of metals and semimetals, and he says, that certain salts, particularly sal ammoniac dissolved in water, give it a considerable power of dispersion. The oxygenated muriatic acid, too, possesses this quality in a great degree; but butter of antimony has a still greater effect, for one prism of this is equal to three of crown glass of similar dimensions. Dr. Blair constructed an object glass with crown glass and butter of antimony, but he observed, that it occasioned irradiations, which led him to prefer oil of turpentine or other essential oils \*.

Grateloup used mastic between the glasses,

But change of temperature, and the sea air, injure mastic.

Resin therefore preferable, or turpentine.

Dr. Blair tried various substances; some salts, and oxygenated muriatic acid, had a considerable power of dispersion; and muriate of antimony had still more; but this occasioned irradiations, whence he preferred essential oils.

The use of achromatic glasses applied to graduate circles or segments of circles for measuring angles, and the defects to which instruments of this kind are liable, may be passed over as not to the present purpose, which is the best mode of measuring very small angles.

Buffon, who had paid some attention to the formation of rock crystals, finding no indication of a double refraction in the experiments which I made on that of Madagascar, in 1770, thought this transparent quarter to be of a different nature from the other crystals; but before he made up his mind on the subject, he desired me to examine it afresh with a view to this property.

Buffon suspected the double refraction of the rock crystal of Madagascar might have been overlooked.

\* An ample abridgement of Dr. Blair's paper is given, Phil. Journal, 4to, I. 1.



Beccaria published a curious work on the refraction of crystals.

Huygens mistaken in calling the refraction irregular ;

as it uniformly depends on the internal structure of the crystal.

Beccaria cut rock crystal in different directions ;

whence he obtained certain laws of their refraction.

It may not be improper here to give a brief abstract of a very curious work, which Beccaria published in Italian about this time, as I learned from it the direction in which it was necessary to cut my prisms, to be certain of obtaining the effects I sought. Huygens, says Beccaria, who treats very fully on the double refraction of Iceland crystal, adds, that rock crystal possesses the same property, but in a less sensible degree. I saw that the refraction which Huygens calls irregular, bears a constant relation in the Iceland crystal to its internal structure ; for the effect of this refraction is to divert the rays in the direction of the salient angles, which are equal in the whole piece, and in each of its component parts. From inspecting the figure of rock crystal, I inferred its internal structure. In fact, I considered it as similar to artificial crystallizations, formed by the aggragation of little laminæ parallel to the faces of the crystals. Suspecting Huygens of inaccuracy in asserting, that prisms of rock crystal had always a double refraction, whatever their sections were, I cut a piece of rock crystal, of a very irregular figure, in the direction of its axis, and so as to divide two of its opposite faces into two equal parts. I cut another likewise in the direction of its axis, but so that the section passed through the summits of two opposite angles. A third afforded me triangular prisms, one of the faces of which was one of the faces of the crystal itself, while each of the other two faces terminated on one side at one of the contiguous angles, and on the other at the axis of the crystal. A fourth was divided into equilateral prisms, two of the faces of which were equally inclined to the axis, and the third was parallel to a plane continued through the axis. From the observations made with these prisms, I obtained a very simple law, of which Newton says nothing, and the contrary of which is advanced by Huygens. 1. That the double refraction does not subsist in all the different prisms that may be cut from rock crystal, 2. That the ray of light which traverses rock crystal in a place perpendicular to the axis undergoes two refractions, is divided into two, and gives two images, if not completely yet sensibly distinct. 3. That this distinctness of the two images diminishes in proportion as the direction of the ray approaches to that of the axis of the crystal. 4. That the double refraction and dispersion of the two images cease entirely, when the direction of the ray is parallel to the axis, or nearly parallel to it.



it. Hence it follows, that in cutting a lens of rock crystal for optical purposes, we must take pieces parallel to the base of the crystal, so that the axis of the lens coincides with that of the crystal, or is at least parallel to it. Thus far Beccaria.

The refraction of the crystal is single in the direction of its axis.

When we would apply to practice this properly in rock crystal of doubling the images of objects, the colours that arise from the prismatic figure necessary to be given to it must be corrected by a glass prism, or the images will not appear well defined.

A prism of rock crystal must be corrected by one of glass:

These two prisms united together may be called a *double refracting achromatic medium*, but in this state it can serve only to measure the small angle given by the double refraction: yet it is easy to conceive, that on applying two

the two forming a double refracting achromatic medium.

double refracting mediums one on the other, we may vary the effect of the double refraction at pleasure by a circular movement, as in the instrument for measuring colours which I laid before the Academy. Thus this double refraction, which has been deemed detrimental to the construction of optical instruments, is in fact advantageous for the mensuration of small angles, as I convinced the Academy, February the 25th, 1777, by an instrument constructed on this principle, which gave the measure of small angles with a degree of precision, for which we could scarcely have hoped. This instrument,

Instrument for measuring small angles by means of two of these mediums.

however, had the inconvenience of giving four images of one object, which occasioned a considerable loss of light; and the less light we have, the less the accuracy with which the point of contact can be observed.

Its defect.

Endeavouring to remedy this imperfection, I soon discovered a more simple construction for the instrument, which I laid before the Academy the same year. In this only one double refracting achromatic medium is required, which is made to move along the inside of any telescope in the axis of the object glass. The value of the double refraction is first to be determined by experiment, as in the former micrometer, the achromatic medium being placed against the object glass. It is then to be moved from the object glass toward the eye glass. The angle of the double refraction will now be what it was in the former situation, but the separation of the images will be in the ratio that the distance of the refractive medium from the focus bears to the focal distance. Suppose for example the double refraction to be twenty-one minutes, and the focal distance of the object glass to be three metres: if the

This remedied by using one medium, made moveable along the axis of the telescope.

Mode of using  
this micrometer.

achromatic medium be brought nearer by two thirds of this distance, the double refraction will occasion only a separation of seven minutes between the images. Thus if we would measure the diameter of an object, we must move the achromatic medium toward the focus, till the two images of the object are seen precisely in contact; when, having the angle of double refraction given, by previous experiment, the focal distance of the object glass, and the distance of the achromatic medium from the focus, the diameter will be given by the rule of proportion. It is obvious, that great care must be taken in determining the angle of double refraction at first with precision, as on this will depend the accuracy of all subsequent measures with the instrument.

The instrument  
farther im-  
proved by using  
two prisms of  
rock crystal cut  
in opposite di-  
rections.

The effect of  
the double re-  
fraction dou-  
bled,

by means of  
two equal  
prisms.

This instrument  
difficult to exe-  
cute.

Hence the au-  
thor was led to  
avail himself of  
Euler's method.  
Effect of the

Having thus improved the construction of this micrometer, I found that the glass prisms, intended to correct the dispersion of the prism of rock crystal, left a refraction more and more perceptible in proportion as the double refracting medium was carried farther from the eye glass. I then availed myself of Beccaria's discovery, and cut my two prisms so that the first was in the direction of the double refraction, and the second in that in which the double refraction is not perceptible: by these means I had a double refracting medium absolutely exempt from colours and refractions. I did not stop here, for I was desirous of extending the effect of the double refractions so as to measure the diameter of the sun; and I accomplished this, which appeared to exceed the known power of the double refraction of rock crystal, which does not go beyond twenty minutes, when it is cut in the most advantageous prismatic shape. For this I employed two equal prisms, cut in the direction most suitable to my purpose; and on placing them in opposite directions I found, that the double refraction was not perceptible; but on reversing their directions the double refraction of each prism was nearly doubled, so that I obtained two images separated by an interval of forty minutes.

I ought not to omit, that in this new construction there are difficulties of execution not easy to surmount, which may have been one reason why these instruments, so useful to navigators and in certain very nice astronomical observations, have not been adopted. This induced me at length to adopt Euler's method. In the construction of achromatic object glasses I found I could increase or diminish the absolute effect of the

double



double refraction within certain limits, by means of the interval between the glasses of differing refracting powers: the separation of the images at the focus being so much the greater, as the interval is larger, when the flint glass is the first of the object glasses; and less, when it is the second.

Conformably to these new principles I have had two telescopes with a double refracting medium constructed under my own inspection, which General Gauthaume will employ for determining the position of his ships, and to find whether he be approaching any he may meet with at sea\*.

The uses of an instrument for measuring very small angles with precision are too well known for me to describe its advantages. The officers of the English navy are so fully aware of them, that they have used for some years Ramsden's eye glass micrometer, though this answers the end but imperfectly, because it does not give the measure of the angle, and because of the bad effect of the parallax produced by the decussation of the rays that enter the eye. This defect is more sensible in Ramsden's eye glass micrometer than in Bouguer's heliometer. The officers who have compared my instrument with Ramsden's, of which there were several on board the Spanish ships with our Brest fleet, agree that the celebrated English artist has very imperfectly accomplished the object he proposed; and Bouguer's heliometer could unquestionably be preferable for naval use, because it has a less sensible parallax, and gives the measure of small angles, so important for determining the distances of ships from their known dimensions.

*Table of the proportions which the magnitude of an object bears to its distance: calculated according to the rule, which is easily demonstrated, that, in every right angled triangle, the tangent is to the radius as the magnitude of an object is to its distance from the centre of the eye, when, under an angle known, the distance forms a right angled triangle with the object.*

\* The General has made an advantageous report of this instrument, in his account of the chase of the *Swiftsure*, which he captured. This instrument is so difficult to execute, that I know only one person, Citizen *Narci*, who is capable of giving rock crystal the prismatic form in the proper direction for obtaining the double refractions necessary to the goodness of the micrometer. He has made several, which have given me perfect satisfaction.

EXAMPLE.



## EXAMPLE.

General formula  
for calculating  
distances.

Suppose the angle be measured under which we perceive an object of the magnitude of one toise, placed vertically so as to represent the tangent, the radius of which is the distance from the eye; if this angle be of  $30'$  we shall have the following proportion: the tangent of  $30'$  is to the radius, as one toise is to the distance. By the tables of logarithms we shall have this distance by subtracting from the logarithm of the cosine the logarithm of the tangent of  $30'$ , which is 7.940858: the logarithm resulting from this subtraction is 2.059142; and the number answering to this logarithm is 114.6 nearly; consequently the distance from the eye to the object, the magnitude of which is supposed to be one toise, is 114 toises six tenths; but in the use of the instrument fractions may be neglected.

It is proper to observe, that in measuring small angles the triangle need not be perfectly rectangular: the object may sensibly vary from the perpendicular, without a perceptible difference in the angle.

## QUESTION I.

Mode of determining by the micrometer the distance of an object of known magnitude.

*The magnitude of an object being known, to determine its distance.*

For this purpose the two images of the object produced by the prisms are to be brought into contact, by moving the index along the axis that carries them. This rule has two divisions, one shewing the minutes and seconds, the other the proportion between the diameter of the object and the distance. Suppose the object I look at is a man: the mean height of a man with his hat on may be estimated at six feet. Having disposed the prisms and telescope so that the two images are in one vertical line, I move the index till the feet of the upper image come into contact with the summit of the lower: if now the index point at the number 344, I conclude that the distance of the man from my eye is 344 times six feet, or 344 toises. The second division will shew, that the angle under which I see the man is  $10'$ . By this method the measure of a distance may unquestionably be obtained with great precision, but there are many cases, in which this great precision is not necessary.

Ships,

Ships, buildings in which certain rules little liable to variation are observed, and the dimensions of different animals, may serve to give this distance. By means of such objects I have often determined inaccessible distances with truly surprising celerity, by means of a portable telescope with prisms, which requires no support for taking angles. When greater accuracy is required, objects of which the diameter is well ascertained must be employed.

If lighthouses had on them a cross of fixed and known dimensions, it would thus be of great service to navigators: for by the angle of the perpendicular piece, given by this micrometer, the distance of the ship from the lighthouse would be ascertained; and by that of the horizontal piece would be known the position of the ship with respect to the coast.

Crosses of known dimensions on light-houses would be of great utility to navigation.

### QUESTION II.

*The distance of an object being given, to determine its magnitude.*

This is precisely the reverse of the preceding.

The magnitude of an object determined from the distance known.

### QUESTION III.

*The magnitude and distance of an object being unknown, to determine both.*

Suppose the object makes an angle of  $40'$  with my eye; as this angle will increase in regular proportion as the distance is diminished, I approach the object till the angle is  $41'$ , a difference which will be sufficient in ordinary cases. If I find the distance between the two stations, on measuring it, to be a hundred toises, I shall obtain the whole distance by multiplying this by the number of minutes of the larger angle, and dividing the product by the difference between the two angles, which will give in this instance 4100 toises. The distance being thus obtained, the magnitude of the object will be found by dividing 4100 toises by  $83\frac{1}{2}$ , the number answering to  $41'$ ; which will give the magnitude forty-nine toises seven inches nearly.

Method of determining the magnitude and distance, where both are unknown.

## XI.

*Observations on the two lately discovered celestial Bodies. By*  
 WILLIAM HERSCHEL, LL. D. F. R. S.

The moving star  
 of Piazzi is very  
 minute.

IN my early account of the moving star discovered by Mr. Piazzi, I have already shewn that it is of a remarkably small size, deviating much from that of all the primary planets \*.

It was not my intention to rest satisfied with an estimation of the diameter of this curious object, obtained by comparing it with the Georgian planet, and, having now been very successful in the application of the lucid disk micrometer, I shall relate the result of my investigations.

Another discovered by Dr. Olbers.

But the very interesting discovery of Dr. Olbers having introduced another moving star to our knowledge, I have extended my researches to the magnitude, and physical construction, of that also. Its very particular nature, which, from the observations I shall relate, appears to be rather cometary than planetary, will possibly throw also considerable light upon the circumstances belonging to the other celestial body; and, by that means, enable us to form some judgment of the nature of both the two last discovered phenomena.

As the measures I have taken will oblige me to give a result which must appear extraordinary, it will be highly necessary to be particular in the circumstances of these measures, and to mention the condition and powers of the telescopes that were used to obtain them.

*Magnitude of the new Stars.*

Observations on  
 Ceres compared  
 with a lucid  
 disk.

7-feet reflector.

April 1, 1802. Having placed a lucid disk at a considerable distance from the eye, but so that I might view it with perfect distinctness, I threw the image of Mr. Piazzi's star, seen in a 7-feet reflector, very near it, in order to have the projected picture of the star and the lucid disk side by side, that I might ascertain their comparative magnitudes. I soon perceived that the length of my garden would not allow me to remove the disk-micrometer, which must be placed at right

\* By comparing its apparent disk with that of the Georgian planet, it was estimated, that the real diameter of this new star could not amount to three-eighths of that of our moon.

angles



angles to the telescope, far enough to make it appear no larger than the star; and, not having disks of a less diameter prepared, I placed the smallest I had as far from me as the situation of the star would allow. Then, bringing its image again by the side of the disk, and viewing, at the same time, with one eye the magnified star, while the other eye saw the lucid disk, I perceived that Ceres, which is the name the discoverer has given to the star, was hardly more than one third of the diameter of the disk, and certainly less than one half of it.

This being repeated, and always appearing the same, we shall not under-rate the size of the star, by admitting its diameter to have been 45 hundredths of the lucid disk.

The power of the telescope, very precisely ascertained by terrestrial geometrical measures properly reduced to the focus of the mirror on the stars, was 370,42. The distance of the lucid disk from the eye, was 2131 inches; and its diameter 3,4 inches. Hence we compute, that the disk was seen under an angle of  $5' 29''.09$ ; and Ceres, when magnified 370 times, appearing, as we have shewn, 45 hundredths of that magnitude, its real diameter could not exceed  $0''.40$ . Had this diameter amounted to as much as was formerly estimated, the power of 370 would have made it appear of  $6' 10''$ , which is more than the whole lucid disk.

Diameter of Ceres  $0''.40$ .

This extraordinary result raised in me a suspicion, that the power 370 of a 7-foot telescope, and its aperture of 6,3 inches, might not be sufficient to shew the planet's feeble light properly. I therefore adapted my 10-foot instrument to observations with lucid disks; which require a different arrangement of the head of the telescope and finder: I also made some small transparencies, to represent the object I intended to measure.

April 21. The night being pretty clear, though perhaps not quite so proper for delicate vision as I could have wished, I directed my 10-foot reflector, with a magnifying power of 516,54, also ascertained by geometrical terrestrial measures reduced to the focus of the instrument on celestial objects, to Mr. Piazzi's star, and compared it with a lucid disk, placed at 1486 inches from the eye, and of 1,4 inch in diameter. I varied the distance of the lucid disk many times; and fixed at last on the above-mentioned one, as the best I could find. There was, however,

2. Observation repeated on Ceres with 10-foot reflector. Power 516,54.

Hazinefs.

however, a hazinefs about the star, which refembled a faint coma; and this, it may be fupposed, muft render the meafure lefs fatisfactory than it would otherwife have been.

Diameter of Ceres 0",38.

From thefe data we compute, that the difk appeared to the natural eye under an angle of  $3' 14'',33$ ; while Ceres, when magnified  $516\frac{1}{2}$  times, was feen by the other eye of an equal magnitude; and that confequently its real diameter, by meafurement, was only 0",38.

3. Observation repeated on Ceres; 10-feet reflector, new small mirror;

April 22. 11<sup>h</sup> 38', fidereal time. I ufed now a more perfect fmall mirror; the former one having been injured by long continued folar obfervations. This gave me the apparent diameters of the ftars uncommonly well defined; to which, perhaps, the very favourable and undifturbed clearnefs of the atmofphere might contribute confiderably.

power 881,51;

With a magnifying power of 881,51, properly afcertained, like thofe which have been mentioned before, I viewed Dr. Olbers's ftar, and compared it with a lucid difk of 1,4 inch in diameter, placed at 1514 inches from the eye, meafured, like the reft of the diftances, with long deal rods. The ftar appeared to me fo ill defined, that, afcribing it to the eye-glafs, I thought it not advifeable to compare the object, as it then appeared, with a well defined lucid difk. Exchanging the glafs for that which gives the telescope a magnifying power of  $516\frac{1}{2}$ , I found Pallas, as the difcoverer wifhes to have it called, better defined; and faw, when brought together, that it was confiderably lefs in diameter than the lucid difk.

bad obf.

Power changed to  $516\frac{1}{2}$ .

Pallas,

In order to produce an equality, I removed the difk to 1942 inches; and ftill found Pallas confiderably lefs than the difk.

fmaller than Ceres.

Before I changed the diftance again, I wifhed to afcertain whether Ceres or Pallas would appear under the largeft angle, efpecially as the air was now more pure than laft night. On comparing the diameter of Ceres with that of the lucid difk, I found it certainly lefs than the difk. By proper attention, and continued examination, for at leaft an hour, I judged it to be nearly  $\frac{3}{4}$  of the lucid difk.

Then, if we calculate as before, it appears by this obfervation, in which there is great reafon to place confidence, that the angle under which this ftar appeared, was only 0'.22. For, a lucid difk of 1,4 inch diameter, at the diftance of 1942



inches, would be seen under an angle of  $2' 28'', 7$ ; three quarters of which are  $1' 51'', 52$ . This quantity, divided by the power 516,54, gives  $0'', 2159$ , or, as we have given it abridged,  $0'', 22$ . Excellent result of Ceres, diam.  $0'', 22$ .

$13^h 7'$ . I removed the micrometer to the greatest convenient distance, namely, 2136 inches, and compared Dr. Olbers's star, which, on account of its great altitude, I saw now in high perfection, with the lucid disk. It was, even at this distance, less than the diameter of the disk, in the proportion of 2 to 3.

When, by long continued attention, the appearance of Pallas was reduced to its smallest size, I judged it to bear no greater proportion to the diameter of the lucid disk of the micrometer, than as 1 to 2.

In consequence of these measures, it appears that the diameter of Pallas, according to the first of them, is  $0'', 17$ ; and, according to the last, where the greatest possible distinctness was obtained, only  $0'', 13$ . Diam. of Pallas  $0'', 13$ .

If it should appear almost incredible that these curious objects could give so small an image, had they been so much magnified as has been reported, I can say, that curiosity led me to throw the picture of Jupiter, given by the same telescope and magnifying power, on a wall at the distance of 1318 inches, of which it covered a space that measured 12 feet 11 inches. I do not mention this as a measure of Jupiter, for the wall was not perfectly at right angles to the telescope, on which account the projected image would be a little larger than it should have been, nor was I very attentive to other necessary minute circumstances, which would be required for an accurate measure; but we see at once, from the size of this picture, that the power of the telescope exerted itself to the full of what has been stated. Proof by the image of Jupiter that the power of the telescope was truly as here stated.

As we generally can judge best of comparative magnitudes, when the measures are, as it were, brought home to us; it will not be amiss to reduce them to miles. This, however, cannot be done with great precision, till we are more perfectly acquainted with the elements of the orbits of these stars. But, for our present purpose, it will be sufficiently accurate, if we admit their mean distances from the sun, as the most recent information at present states them; for Ceres 2,6024; and for Pallas 2,8. The geocentric longitudes and north latitudes, at the time Rough computation of the diameters of these bodies in miles:

of



of observation, were, for Ceres, about  $\text{N} 20^{\circ} 4', 15^{\circ} 20'$ ; and for Pallas,  $\text{N} 23^{\circ} 40', 17^{\circ} 30'$ . With these data, I have calculated the distances of the stars from the earth at the time of observation, partly by the usual method, and, where the elements were wanting, by a graphical process, which is sufficiently accurate for our purpose. My computed distances were 1,634 for Ceres, and 1,8333 for Pallas; and, by them we find, that the diameter of Ceres, at the mean distance of the earth from the sun, would subtend an angle of  $0'', 35127$ ; and that, consequently, its real diameter is 161,6 miles.

That of Ceres  
proves to be  
161,6 miles;  
and that of Pallas  
147 or  $110\frac{1}{2}$   
miles.

It also follows, that Pallas would be seen, at the same distance from the sun, under an angle of  $0'', 3199$ ; and that its real diameter, if the largest measure be taken, is 147 miles; but, if we take the most distinct observation, which gives the smallest measure, the angle under which it would be seen from the sun, will be only  $0'', 2399$ ; and its diameter, no more than  $110\frac{1}{2}$  miles.

#### *Of Satellites.*

Examination for  
discovering  
Satellites.

None were  
ascertained.

After what has just now been shewn, with regard to the size of these new stars, there can be no great reason to expect that they should have any satellites. The little quantity of matter they contain, would hardly be adequate to the retention of a secondary body; but, as I have made many observations with a view to ascertain this point, it will not be amiss to relate them.

Feb. 25. 20-feet reflector. There is no small star near Ceres, that could be supposed to be a satellite.

Feb. 28. There is no small star within 3 or 4 minutes of Ceres, that might be taken for a satellite.

March 4.  $9^{\text{h}}. 45'$ . sidereal time. A very small star, south-preceding Ceres, may be a satellite. See Plate V. Fig. 1. where C is Ceres, S the supposed satellite,  $a b c d e f$ , are delineation stars,  $c$  and  $d$  are very small. S makes nearly a right angle with them;  $e$  is larger than either  $c$  or  $d$ . There is an extremely faint star  $f$ , between  $e$  and  $d$ .

$14^{\text{h}}. 16'$ . Ceres has left the supposed satellite behind.

March 5. There are two very small stars, which may be satellites; see Fig. 2. where they are marked, 1st S, 2d S. The rest, as before, are delineation stars.

March 6. The two supposed satellites of last night remain in their situation, Ceres having left them far behind.

10<sup>h</sup>. 16'. There is a very small star, like a satellite, about 75° south-following Ceres. See Fig. 3. It is in a line from C to *b* of last night.

11<sup>h</sup>. 20'. Ceres has advanced in its orbit; but has left the supposed satellite behind.

March 30. 9<sup>h</sup>. 35'. A supposed 1st satellite is directly following Ceres: it is extremely faint. A 2d supposed satellite is north-following. See Fig. 4. The supposed satellites are so small, that, with a 20-feet telescope, they require a power of 300 to be seen; and the planet should be hidden behind a thick wire, placed a little out of the middle of the field of view, which must be left open to look for the supposed satellites.

12<sup>h</sup> 17'. Ceres has changed its place, and left both the supposed satellites behind.

March 31. 9<sup>h</sup> 20'. There is a very small star, on the north-preceding side of Ceres, which may be a satellite.

11<sup>h</sup> 50'. Ceres has moved forwards in its path; but the supposed satellite remains in its former situation. The nearest star is 20'' of time from Ceres; so that, within a circle of 40'' of time, there certainly is no satellite that can be seen with the space-penetrating power of this instrument.

It is evident, that when the motion of a celestial body is so considerable, we need never be long in doubt whether a small star be a satellite belonging to it, since a few hours must decide it.

May 1. 12<sup>h</sup> 51'. I viewed Pallas with the 20-feet reflector, power 300; there was no star within 3', that could be taken for a satellite.

#### *Of the Colour of the new Stars.*

- |           |  |                             |
|-----------|--|-----------------------------|
| Feb. 13.  | The colour of Ceres is ruddy, but not very deep. | Ceres is of a ruddy colour; |
| April 21. | Ceres is much more ruddy than Pallas.            | Pallas dusky                |
| April 22. | Pallas is of a dusky whitish colour.             | white.                      |

#### *Of the Appearances of the new Stars, with regard to a Disk.*

- |          |  |                                   |
|----------|--|-----------------------------------|
| Feb. 7.  | Ceres, with a magnifying power of 516½, shews an ill defined planetary disk, hardly to be distinguished from the surrounding haziness. | The disk of Ceres is ill-defined; |
| Feb. 13. | Ceres has a visible disk.  |                                   |

April

Pallas is not a disk but a nucleus.

April 22. In viewing Pallas, I cannot, with the utmost attention, and under the most favourable present circumstances, perceive any sharp termination which might denote a disk; it is rather what I would call a nucleus.

April 28. In the finder, Pallas is less than Ceres. It is also rather less than when I first saw it.

*Of the Appearances of the new Stars, with regard to an Atmosphere, or Coma.*

Ceres has most frequently a coma (perhaps effected by the instrument, or more probably by the atmosphere.)

April 21. I viewed Ceres for nearly an hour together. There was a haziness about it, resembling a faint coma, which was, however, easily to be distinguished from the body.

April 22. I see the disk of Ceres better defined, and smaller than I did last night. There does not seem to be any coma; and I am inclined to ascribe the appearance of last night to a deception, as I now and then, with long attention, saw it without; at which times it was always best defined, and smallest.

April 28. Ceres is surrounded with a strong haziness. Power 550.

With 516 $\frac{1}{2}$ , which is a better glass, the breadth of the coma beyond the disk may amount to the extent of a diameter of the disk, which is not very sharply defined. Were the whole coma and star taken together, they would be at least three times as large as my measure of the star. The coma is very dense near the nucleus; but loses itself pretty abruptly on the outside, though a gradual diminution is still very perceptible.

April 30. Ceres has a visible, but very small coma about it. This cannot be seen with low powers; as the whole of it together is not large enough, unless much magnified, to make up a visible quantity.

May 1. The diameter of the coma of Ceres, is about 5 times as large as the disk, or extends nearly 2 diameters beyond it.

13<sup>h</sup> 19'. 20-feet reflector; power 477. The disk of Ceres is much better defined than that of Pallas. The coma about it is considerable, but not quite so extended as that of Pallas.

May 2. 13<sup>h</sup> 20'. Ceres is better defined than I have generally seen it. Its disk is strongly marked; and, when I see it best, the haziness about it hardly exceeds that of the stars of an equal size.

*Memorandum.*



*Memorandum.* This may be owing to a particular disposition of the atmosphere, which shews all the stars without twinkling, but not quite so bright as they appear at other times. Jupiter likewise has an extremely faint scattered light about it, which extends to nearly 4 or 5 degrees in diameter.

April 22. Pallas, with a power of  $881\frac{1}{2}$ , appears to be very ill defined. The glass is not in fault; for, in the day time, I can read with it the smallest letters on a message card, fixed up at a great distance. Pallas is ill-defined, nebulous or cometary,

13<sup>h</sup> 17'. The appearance of Pallas is cometary; the disk, if it has any, being ill defined. When I see it to the best advantage, it appears like a much compressed, extremely small, but ill defined, planetary nebula.

April 28. Pallas is very ill defined: no determined disk can be seen. The coma about it, or rather the coma itself, for no star appears within it, would certainly measure, at first sight, 4 or 5 times as much as it will do after it has been properly kept in view, in order to distinguish between the haziness which surrounds it, and that part which may be called the body.

May 1. Pallas has a very ill defined appearance; but the whole coma is compressed into a very small compass.

13<sup>h</sup> 5'. 20-feet reflector; power 477. I see Pallas well, and perceive a very small disk, with a coma of some extent very small disk. about it, the whole diameter of which may amount to 6 or 7 times that of the disk alone.

May 2. 13<sup>h</sup> 0'. 10-feet reflector. A star of exactly the same size, in the finder, with Pallas, viewed with  $516\frac{1}{2}$ , has a different appearance. In the centre of it is a round lucid point, which is not visible in Pallas. The evening is uncommonly calm and beautiful. I see Pallas better defined than I have seen it before. The coma is contracted into a very narrow compass; so that perhaps it is little more than the common aberration of light of every small star. See the memorandum to the observations of Ceres, May 2.

#### *On the Nature of the new Stars.*

From the account which we have now before us, a very important question will arise, which is, What are these new stars, are they planets, or are they comets? And, before we

can

## Criteria of planets.

1. Magnitude.

2. Orbits nearly circular.

3. Nearly in the pl. ecliptic.

4. Motion direct.

5. Satellites or rings.

6. Atmosphere, not extensive.

7. Orbits considerably asunder.

Comparison of the new stars in these respects.

can enter into a proper examination of the subject, it will be necessary to lay down some definition of the meaning we have hitherto affixed to the term planet. This cannot be difficult, since we have seven patterns to adjust our definition by. I should, for instance, say of planets,

1. They are celestial bodies, of a certain very considerable size.

2. They move in not very excentric ellipses round the sun.

3. The planes of their orbits do not deviate many degrees from the plane of the earth's orbit.

4. Their motion is direct.

5. They may have satellites, or rings.

6. They have an atmosphere of considerable extent, which however bears hardly any sensible proportion to their diameters.

7. Their orbits are at certain considerable distances from each other.

Now, if we may judge of these new stars by our first criterion, which is their size, we certainly cannot class them in the list of planets: for, to conclude from the measures I have taken, Mercury, which is the smallest, if divided, would make up more than 135 thousand such bodies as that of Pallas, in bulk.

In the second article, their motion, they agree perhaps sufficiently well.

The third, which relates to the situation of their orbits, seems again to point out a considerable difference. The geocentric latitude of Pallas, at present, is not less than between 17 and 18 degrees; and that of Ceres between 15 and 16; whereas, that of the planets does not amount to one half of that quantity. If bodies of this kind were to be admitted into the order of planets, we should be obliged to give up the zodiac; for, by extending it to them, should a few more of these stars be discovered, still farther and farther deviating from the path of the earth, which is not unlikely, we might soon be obliged to convert the whole firmament into zodiac; that is to say, we should have none left.

In the fourth article, which points out the direction of the motion, these stars agree with the planets.

(To be continued in our next.)

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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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MARCH, 1803

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ARTICLE I.

*Comparative Experiments and Observations on Myrtle Wax, Bees Wax, Spermaceti, Adipocire, and the Crystalline Matter of biliary Calculi. In a Letter from JOHN BOSTOCK, M. D.*

To Mr. NICHOLSON.

Liverpool, Jan. 23, 1803.

SIR,

IN the course of last summer, I was requested by a friend, who had received from America a quantity of the peculiar vegetable matter called Myrtle Wax, to give him some information respecting its nature and properties, and the probable use to which it might be advantageously applied. For this purpose I examined several works on natural history and chemistry, but I could only meet with a general account of the method employed for obtaining it, with a vague description of its physical properties. Respecting its chemical analysis I could find nothing satisfactory; even the elaborate work of Fourcroy contains little precise information on this head \*. Enquiry concerning the nature and uses of myrtle wax.

\* Fourcroy, *Système des Connoissances Chimiques*, tom. vii. p. 339.

*I Scarcely mentioned by Fourcroy.*



But more fully  
by Cadet.

determined therefore to examine this point more accurately, and with this intention, the experiments of which I send you an abstract were performed. Some other subjects however having at that time occupied me, I had almost forgotten my analysis of the myrtle wax, until in looking over the 131st No. of the *Annales de Chimie*, I found that the subject had been treated in an ample manner by M. Cadet. This circumstance induced me to recur to my Papers, in order to compare my experiments with those of M. Cadet; when I found that though his memoir contains a very complete account of the natural history of the substance, yet that my chemical analysis was more minute. On this account I conceive, that if you have any intention of inserting in your Journal \* a translation of M. Cadet's essay, my communication may prove a useful supplement.

I am,

Your obedient servant,

JOHN BOSTOCK.

*Description and Analysis of Myrtle Wax.*

Obvious properties of myrtle wax.

The peculiar substance obtained from the *Myrica Cerifera* of Linnæus †, and which has obtained the name of myrtle wax, is a concrete body, of moderate hardness and consistence; it has in part the tenacity of bees wax, though without its unctuousity; along with this, it also possesses, in some degree, the brittleness of the resins. The colour of the myrtle wax is a pale green; the shades of the different pieces are however somewhat varied; in most of them, the green has a tendency to a dirty grey; in others, it is lighter, more transparent, and of a yellowish tinge. Its specific gravity is about 1,0150, water being 1,0000, and white bees wax, 9600. It is fused at a temperature of 109°; by sufficiently increasing the heat, it burns with a peculiarly clear, white flame, produces little smoke, and during the combustion emits an agreeable aromatic odor.

Habitudes with reagents.  
Water has no action.

The following are its habitudes with the different re-agents.

I. Water has no action upon the myrtle wax, either when cold or at the boiling heat.

\* The reader will find it in the present Number. N.

† Martyn's edition of Millar's Dictionary.

Alcohol,

2. Alcohol, at the ordinary temperature of the atmosphere, has no action upon it; but 100 parts, by weight, of this fluid, when boiling, dissolve about five parts of the wax. Nearly  $\frac{4}{5}$  is deposited by the cooling of the alcohol;  $\frac{1}{5}$  still remains suspended; this is slowly deposited in the course of a few days, or may be precipitated by the addition of water. This substance, when precipitated from the alcohol, is lighter coloured than in its original state, and approaches more to a grey tinge. Though the myrtle wax seems to be homogeneous in its texture and consistence, it is not totally soluble in alcohol: about  $\frac{4}{5}$  only of the whole is acted upon by this fluid, even when boiling. The part which is insoluble in alcohol, when separated from the rest of the mass, exhibits a somewhat darker shade of green than before the experiment; the alcohol remains completely colourless.

3. Sulphuric ether, when at the common temperature of the atmosphere, dissolves the myrtle wax only in small quantity, but it acts upon it rapidly when boiling. On account of the tendency which the fluid has to evaporate, it is difficult to ascertain the exact proportion, but it seems to take up somewhat more than  $\frac{1}{4}$  of its own weight. The greatest part of this is separated as the ether cools, and the rest may be precipitated by the addition of water. The wax, after it has been dissolved in ether, is nearly colourless, while the fluid assumes a beautiful green hue. If the wax be not too abundant, and the ether be suffered to evaporate slowly, the wax is deposited on the sides of the glass in a crystalline or lamellated form; in this state its texture approaches somewhat to that of spermaceti.

4. Rectified oil of turpentine at the temperature of the atmosphere softens the wax, but does not seem capable of dissolving it; when assisted by a moderate heat, 100 grains of the turpentine dissolve six grains of the myrtle wax. The turpentine acquires a light green tinge, part of the wax is separated as the fluid cools, while part remains permanently dissolved in it.

5. When the myrtle wax is boiled with liquid caustic potash, the fluid becomes turbid, but after some time the greatest part of the wax rises to the surface, nearly without colour, in a flocculent form. A small quantity of it only remains dissolved.

Cold alcohol does not act upon it. Hot alcohol sparingly dissolves it in part only, and deposits this by cooling and repose.

Cold S. Ether dissolves little; hot ether more rapidly and largely. The deposit by evaporation resembles spermaceti.

Ol. Turpentine acts but slightly.

Pure potash in water renders it colourless by boiling, and forms a soap with a small part, which be-

ing decomposed  
by acid affords  
the wax nearly  
unchanged.

Pure ammonia  
acts nearly as  
potash, but  
more feebly.

The three prin-  
cipal mineral  
acids act upon  
it; though not  
with any notable  
force.

Deduction :  
that myrtle wax  
appears to be an  
oxygenated  
fixed oil.

solved in the potash; this may be precipitated from it by an acid. That part of the wax which rises to the surface is converted into a saponaceous matter; it has lost its inflammability and fusibility, and forms an opaque solution with water. From this solution it is precipitated by an acid in the form of white flakes, which when collected, will be found to resemble very nearly the wax before its union with the potash.

6. Pure ammonia exhibits with the myrtle wax phenomena in many respects, similar to those produced by the fixed alkalis. When its action is promoted by heat, an opaque solution is produced; the wax is deprived of its colour; the greatest part of it separates from the fluid, and is converted into a substance partly soluble in warm water, though less so than that resulting from the action of potash upon myrtle wax.

7. The mineral acids exercise little action upon this substance at the ordinary temperature of the atmosphere; the sulphuric dissolves it sparingly, and acquires a brown tinge. With the assistance of a moderate heat this acid dissolves about  $\frac{1}{2}$  of its weight, and is converted into a thick, dark-brown mass; by cooling it becomes nearly concrete, but no separation of the wax takes place. The nitric and muriatic acids, even when heated, seem to possess little attraction for the myrtle wax. After the wax had been kept for some time in a state of fusion in contact with the nitric acid, its green hue was converted into a pale yellow, but the acid remained colourless, nor did any part of the wax appear to be dissolved in it: the wax by long digestion in muriatic acid became of a bright orange colour.

These experiments will enable us, at least with a considerable degree of probability, to assign the place which the vegetable myrtle wax must hold in a natural arrangement of chemical substances. Its inflammability, fusibility, its insolubility in water, and the action which takes place between it and the alkalis, point out its affinity to the fixed oils, while its texture and consistence, and more particularly its habitudes with alcohol and ether, indicate a resemblance to the resins. We may therefore consider the myrtle wax as a fixed vegetable oil, rendered concrete by the addition of a quantity of oxygen; it seems to hold the same relation to the fixed, that resins do to the essential oils of vegetables.

But



But though the myrtle wax be itself of vegetable origin, there are some animal substances which more nearly resemble it in its chemical properties than any product of the vegetable kingdom. The principal of these is the wax elaborated by the bee, to which the peculiar substance now under consideration bears a strong resemblance, both in its physical and chemical properties. Myrtle wax also in many particulars resembles spermaceti; the substance called Adipocire, produced by the action of nitric acid upon the muscular fibre; and the crystalline matter of biliary calculi. But it resembles the wax of the bee; and also spermaceti; and the adipocire produced by nitric acid upon muscular fibre; and the crystalline matter of biliary calculi.

### *Bees Wax.*

This substance, in its physical properties, differs from myrtle wax in being more unctuous, and possessing a greater degree of tenacity: its colour and smell are also different. Bees wax is likewise considerably less fusible; Dr. Pearson\* and Mr. Nicholson† fix its melting point at 142°; whereas Mr. Fourcroy‡ places it lower in the scale at 117°; on this subject the results of my experiments coincide exactly with those of the English chemists. There is also a difference of opinion among chemists respecting the action which takes place between this substance and alcohol; Fourcroy§, Chaptal||, and Nicholson\*\*, assert that it is insoluble in this fluid, while Pearson maintains the contrary; on this question my experience agrees with that of Dr. Pearson. The proportion of bees wax which the alcohol is capable of dissolving, seems however to be somewhat less than that of the myrtle wax. As in the former case, the greatest part of the wax separates as the fluid cools; while the remainder may be precipitated by the addition of water. Bees wax is sparingly dissolved by boiling ether, less readily, and in considerably less proportion

Comparison with bees wax. Myrtle wax is more unctuous; tenacious; different in colour and smell; more fusible, more soluble in alcohol and in ether; more affected by potash, and nearly the same by ammonia.

\* Pearson's Observations and Experiments on White Lac. Phil. Transf. 1794.

† Nicholson's Journal, Quarto, vol. i. p. 70.

‡ Fourcroy *Système des Connaiss. Chimiques*, x. 343.

§ Ibid. and Thomson's Fourcroy, Vol. iii. p. 387.

|| Chaptal's Elements, Vol. III. p. 164.

\*\* (Nicholson's Elements, p. 502.)

than

than the myrtle wax; this fluid, when heated, seems only to take up about  $\frac{1}{20}$  of its weight of bees wax. Caustic potash exhibits the same phenomena with bees wax as with the product of the *myrica cerifera*; it was converted into the saponaceous state, and became soluble in warm water. It appeared however that the action was less violent, and the change less complete than in the former case. Ammoniac, when boiling, readily forms with bees wax an emulsion, in some respects resembling that produced by the same substance with the myrtle wax. As the mixture cools, the greatest part of the wax rises to the surface in a flocculent form; it appears to have so far contracted a union with the alkali as to have its texture and odour destroyed, and its fusibility and inflammability diminished; yet it is little, if at all, soluble in water.

### *Spermaceti.*

Spermaceti: Is more fusible than either of the preceding.

Sparingly soluble in alcohol,

rapidly by hot ether,

and oil of turpentine.

More strongly attacked by potash than m. or b. wax.

One property of this substance, which obviously distinguishes it from those already described, is the crystalline texture which it constantly assumes. It is more fusible than either of the substances which we have examined; but respecting the precise temperature at which it becomes liquid, there is a considerable difference of opinion. Fourcroy \* states it to be at the 98th degree, or a little lower †; Mr. Nicholson ‡ supposes it to be at the 133d degree; I have found the melting point of spermaceti to be uniformly 112°; there may perhaps be a real difference in the specimens that have been employed. Like the two kinds of wax, it is soluble in alcohol, though very sparingly; according to my experiments, it required a quantity of boiling alcohol equal to 150 times the weight of the spermaceti to dissolve it; a proportion which nearly coincides with the estimate of Fourcroy: the whole is precipitated as the fluid cools. It is rapidly dissolved by warm ether, by cooling it is precipitated so plentifully, as in appearance to convert the whole into a solid crystallized mass. Spermaceti is also dissolved with great facility by oil of turpentine gently heated, but is deposited from it as it cools. It unites very readily with caustic potash, and the compound is completely

\* Encyc. Math. Chimie, Art. Blanc de Baleine.

† Annales de Chim. tom. vii. 192.

‡ Nicholson's Journal, Vol. I. 4to, p. 70.

soluble

soluble in warm water : potash seems to exercise upon spermaceti a more powerful action, than upon either the myrtle or the bees wax. Ammoniac, at the usual temperature of the atmosphere, does not appear to exercise any action upon spermaceti, but when boiling it unites with it readily, and forms an emulsion, which is not decomposed by the cooling of the mixture, or by the addition of water ; but the spermaceti is instantly precipitated by the addition of an acid. No important phenomena result from the action of the mineral acids upon spermaceti.

Strongly by hot ammonia, and then not decomposed by cooling or dilution, but by an acid.

### *Adipocire.*

I procured a quantity of this substance by digesting diluted nitric acid upon the muscular fibre ; it was afterwards washed in warm water, in order to separate any portion of adhering acid. The matter thus purified, was of a light yellow colour, of about the consistence of tallow, and of a homogeneous texture. Respecting the temperature at which it is fused, we meet with the same uncertainty as in the former cases. Fourcroy in one of his essays \* fixes its melting point at the 98th degree ; the same author in another place states it to be the 110th †, while Mr. Nicholson ‡ supposes it to be as high as the 127th ; in Dr. Rees's Cyclop. § it is stated, that this substance melts at 7° below spermaceti, which according to my estimate would be the 105th degree. In my own experience upon this subject it became liquid at the 92d degree. Alcohol at the ordinary temperature of the atmosphere, dissolves it only in small quantity, but by the assistance of a gentle heat it acts upon it with rapidity. Fourcroy || states that this fluid when boiling dissolves about its own weight of adipocire,  $\frac{1}{4}$  or  $\frac{1}{5}$  of which is retained after the fluid cools. The same chemist in another memoir asserts, that one ounce of alcohol will dissolve 12 drams of this substance \*\*. There may probably be some difference in the chemical nature of adipocire, according

Adipocire. Description.

Fusibility.

Solubility in hot alcohol, considerable.

\* Ann. de Chimie, tom. vii. 192.

† Ann de Chimie, tom. viii. 66.

‡ Nicholson's Journal, ubi supra.

§ Rees's Cyclop. new edit. Art. Adipocire, this is probably inserted only upon the authority of Fourcroy.

|| Ann. de Chimie, VII. 191.

\*\* Ann. de Chimie, VIII. 67.



and in ether.

Alkalis as before.

Ammonia dissolves it cold.

Crystals of biliary calculi.  
Description.

to the process by which it is obtained, or the rapidity of its production; in my experiments the quantity which the alcohol was capable of dissolving, though very considerable, was certainly less than that stated by Fourcroy. The greater part is deposited as the fluid cools, and the remainder may be precipitated by water. The adipocire, after this operation, is rendered nearly white, while the alcohol assumes a deep yellow tinge. Ether dissolves it sparingly, when unassisted by heat; when boiling it takes up about  $\frac{1}{4}$  of its own weight; this is for the most part separated by the cooling of the fluid; the adipocire is deposited nearly white, while the ether acquires a yellowish green colour. The caustic alkalis, both fixed and volatile, exert upon this substance the same kind of action which we have described in the former instances; when heated in contact with it, they form a saponaceous emulsion of a reddish brown colour, which is miscible with water without decomposition. The volatile alkali dissolves it sparingly, without the assistance of heat; a circumstance in which the adipocire differs both from any of the substances which we have hitherto examined, and also from the crystalline matter of biliary calculi. Upon the whole the adipocire is more fusible, more inflammable, and more easily acted upon by the different reagents than any substance which has passed under our review.

#### *Crystalline Matter of Biliary Calculi.*

It now only remained to perform some comparative experiments with the crystalline matter of biliary calculi, and I was fortunately in possession of two of these bodies, which were presented to me by Dr. Gerard of this place. The calculi were similar in their texture and appearance, and of nearly the same size; the one which I examined was of an irregular, polyhedral figure, with its edges and angles blunted; it was somewhat brittle, and of an ochry brown colour. Its specific gravity was about ,9000; it weighed 16 grains. When broken, and viewed through a microscope, it was found to consist of an internal nucleus, formed of radii converging to a centre, and of an external crust composed of four or five thin strata. It evidently consisted of two distinct substances; one white, sparkling, and of a crystalline texture, by which its general structure was determined; the other a number of dark coloured particles, irregularly dispersed through the interstices

of

of the former. I therefore concluded it to be that kind of calculus which Fourcroy calls Cystic-Adipobileous\*; composed of the peculiar crystallized matter, and of particles of inspissated bile, mixed together in different proportions. The crystalline matter is fusible and inflammable, but the precise degree at which it melts has not been ascertained; it is not even softened by the heat of boiling water. Alcohol in the cold has no action upon it, but when boiling it dissolves it with facility; Fourcroy† states, that one part of this substance is taken up by 19 parts of the fluid; this however was not the case with the calculus which I examined; in this instance the alcohol certainly did not dissolve more than  $\frac{1}{30}$  of its weight of the crystalline matter. As the fluid cools, the substance is deposited in the form of white, shining spiculæ, intermixed with thin plates. Ether dissolves it slowly in the cold, but more readily when heated; the greatest part is deposited as the fluid cools, and the rest may be precipitated by water. If the ether be suffered to evaporate slowly at the ordinary temperature of the atmosphere, the matter which it held in solution will be deposited on the sides of the glass, in the form of beautiful radiated crystals. Oil of turpentine acts upon this matter with difficulty; it appears, however, when digested with it for some time at the boiling heat, to dissolve it in a small degree. It is acted upon by caustic potash when boiling, and the result of their union appears to be of the same nature with that described in the former instances. A small quantity is dissolved by the pot-ash, and may be precipitated by an acid; while another part is converted into a substance soluble in water, but insoluble in alcohol; it may be precipitated from the water by an acid. Ammoniac, even when boiling, seems to possess little or no attraction for this crystalline matter. The results of several experiments oblige me to differ from Dr. Powel, respecting the effect of the fixed alcalis upon this peculiar substance; he conceives that it is not acted upon by them‡; the opinion which I have adopted is however supported by the authority of Fourcroy§. Nitric acid, more

Fusible and inflammable; not acted on by water.

Hot alcohol dissolves it sparingly,

ether more fully.

Ol. turpentine little.

Fixed alkalis as before, and ammonia scarcely at all.

Nitric acid acts upon and alters it.

\* Fourcroy, *Système des Conn. Chim.* x. 59.

† *Ann. de Chimie*, VII.

‡ Powel on the Bile, 119.

§ *Encyc. Meth. Chimie*; art. Bile; Fourcroy, *Système des Conn. Chimiques*, x.

Habitudes of the  
altered sub-  
stances.

particularly when assisted by heat, acts readily upon the crystalline matter; during the process there is a disengagement of nitrous gas. A small quantity remains dissolved in the fluid, and may be precipitated from it by potash. The greatest part however rises to the surface as the fluid cools in the form of drops of oil, which gradually grow concrete; the crystalline texture is destroyed, and its consistence resembles that of a vegetable resin. Water does not dissolve this peculiar matter, but it seems to render it somewhat more brittle and friable. Alcohol assisted by a gentle heat dissolved it; it was precipitable from the solution by water in the form of a grey powder. Ether, at the temperature of the atmosphere, dissolved it rapidly; water precipitated it from the ether in the form of drops of oil. When the fluid was evaporated, it was deposited, without exhibiting any marks of a crystalline structure. Caustic potash acted upon it without the assistance of heat, when boiling it dissolved it with more facility; the fluid acquired a reddish brown hue; it was not precipitated by water, but the sulphuric acid separated it in the form of a grey powder. The action of ammoniac was nearly similar, though as I conceived, somewhat more powerful than that of the fixed alcalis. The solution was also of a reddish brown colour; but the precipitate by sulphuric acid was of a bright yellow. It appears therefore that the matter of biliary calculi has its properties materially changed by the operation of the nitric acid. It entirely destroys its crystalline tendency, and renders it more soluble in ether and in the alcalis. It has been supposed that by this process it becomes more assimilated to the adiporefin of the bile \*, but it still differs from it in not possessing any degree of solubility in water †.

The substances  
here treated are  
really different.

These remarks upon the crystalline matter of biliary calculi, shew that there are several important circumstances in which it differs both from spermaceti, and from adipocire, to both of which it has been compared ‡. Upon the whole, though the

\* Powel, p. 121.

† Ann. de Chimie, tom. vii. p. 178. Encyc. Meth. Chimie, art. Bile, p. 566.

‡ I may observe that the dark-coloured particles which were dispersed through the crystals of the calculus, though they might have been originally composed of inspissated bile, did not retain the properties of this substance, as they were nearly insoluble both in boiling water and in alcohol.



five substances which have passed under our review possess certain properties in common, and have a degree of similarity in their external appearance, yet they differ materially in their chemical nature. There is indeed reason to conjecture that they are all composed of the same elements, combined together in different proportions and with different degrees of attraction.

Lavoisier \* first made us acquainted with the chemical composition of oil, and proved that it consists of hydrogen and carbene. This great philosopher also demonstrated that wax differs from oil, in containing a greater proportion of carbene; there is every reason to suppose, that a quantity of oxygen likewise enters into its composition. It may be conjectured, that the five substances which have passed under our review, differ from each other in the proportion of oxygen, hydrogen, and carbene; but the present state of our knowledge will not enable us to determine how far the individual properties will be affected by the different proportions. It had been conjectured, that an addition of carbene renders a body less fusible, and at the same time more soluble in alcohol; but we find from the experiments recited above, that adipocere, which is the most fusible, is likewise the most soluble in alcohol. Probably a good deal may depend in these cases upon the state of the combination of the ingredients, as well as upon their proportions.

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## II.

*On the Nature of Musical Sounds. In Reply to DR. YOUNG.*

*By Mr. JOHN GOUGH.*

TO MR. NICHOLSON.

S I R,

THE controversy between Dr. Young and me has taken a new turn in your last number; for my opponent, with the address of an able politician, endeavours to secure the victory by the aid of a powerful auxiliary. For this purpose, he has attempted to oppress my cause, and silence my reasons, by the authority of an illustrious name; whom he has impressed into

\* Lavoisier, Mem. Acad. Scienc. 1784.

Personal respectability is of weight in history ;

but not in argument or reasoning.

The present controversy not determinable by authority.

his service. The only plea that I can offer in opposition to this formidable confederacy, consists in the following plain remark : I contend for the truth and not for superiority, a motive which obliges every disputant to disregard all authority, except the conviction impressed on the senses by a judicious experiment, and the assent given by the understanding to sound arguments. Notwithstanding the preceding declaration, I acknowledge the weight of personal respectability, in many instances of an experimental nature ; because the reputation of one philosopher stands higher than that of another does, as a conductor of experiments ; on this supposition, the decisions of the former ought to be preferred to those of the latter, when they happen to disagree in their conclusions ; in fact every man's confidence in the justice of an experiment which he has not seen, amounts to nothing more than historical evidence, the credit of the narrative resting entirely upon the character and abilities of the eye witnesses.

The foregoing observations on experimental knowledge have nothing to do with argument ; for every species of true logic, including the mathematics, consists of a train of inductions ; all of which are drawn from maxims, admitted by every party, in a well conducted dispute. On this account, argumentation is not an appeal to a person's faith, but to his rational faculties ; and an unprejudiced judge will prefer the logic of a ploughman to that of Aristotle himself, should the rustic happen to reason better than this master of the art. After what has been said on the weight of authority in philosophical controversy, it will not be expected that I shall pay the least deference to the dogma of any man in the world, unless his opinion is supported by demonstration ; for this is the only literary authority to which an enquirer after the truth ought to bow. Consistently with this declaration, an attempt has been made to demonstrate every proposition that has been advanced on my part, in the present dispute ; a dispute which commenced in the transactions of the Royal Society, which passed from that publication to the Manchester Memoirs, and is now prolonged, Sir, in your Journal. But my care in this respect, and attachment to proof have not been imitated by my opponent ; for nothing appears in the course of the controversy, resembling a refutation of any one of my conclusions, excepting an objection which was offered in Dr. Young's last letter,

to

to the theory of the grave harmonics. The Doctor observes, That two unisons cannot give the octave by bisection, insisted because they cannot form a cycle. I take the liberty to reply, that no two perfect unisons produce a cycle; because the arrangement which is given to their first vibrations, will evidently prevail to the end of the experiment: no points of division can therefore be formed for the separation of the contiguous cycles; in other words, no cycles will be produced, consequently no imaginary sound can be perceived in the course of the Doctor's experiment, because the existence of the grave harmonics depends upon a succession of minute cycles, if my idea of their origin be correct.

The effect of two imperfect unisons will perhaps appear upon examination, not less unfavourable to the Doctor's objection, than the preceding case: consonances of this description, it is true, always run into cycles; but then they are too long to give rise to continuous sounds, being better fitted to produce beats. For instance, let two homogeneous cords of equal diameters be stretched with equal forces; and suppose their lengths to be 20 and 20,1 inches, then the longer will make 200 vibrations, while the shorter compleats 201. But the cord corresponding to the grave harmonic of this consonance, vibrates but once in the same time; therefore the length of it is to 20,1 inches as 200 to 1; or it is 335 feet. The difficulty of conducting such an experiment must be obvious to every one; for whether strings or pipes be used, the lower note of this consonance must compleat at least twelve cycles, or 2400 vibrations in a second; otherwise a grave harmonic cannot result from this combination of imperfect unisons.

Having now replied to the Doctor's objection, apparently in a satisfactory manner, I may without the imputation of rudeness, request him to consider in what way the several properties of these secondary sounds are to be reconciled to his system, more especially their want of a fixed direction: should he fail in the attempt, he needs not be reminded that every theory is imperfect, which explains a part of a phenomenon, but does not embrace the whole of it. The want of direction appeared to me so plain a proof of the nature of the grave harmonics, the first time I made the experiment, that I immediately explained the fact on the principles, which are given at the beginning of this volume of your Journal, without once supposing the theory



to be new ; before Dr. Young's last letter informed me of the circumstance. Every attempt to account for this singular effect of consonances by pulses transmitted to the ear through the air, will in all probability encounter the obstacle already mentioned : for the notion of force comprises the idea of direction, as well as that of power ; and if the latter property cannot be used independent of the former, every theory founded on this principle must be contradicted by observation. I do not pretend to form a judgment on the work of M. Lagrange, from Dr. Young's extracts ; and persons residing at a distance from the metropolis, as I do, seldom enjoy access to uncommon books, such as the *Miscellanea Taurinensia*. However, I agree to the proposition of M. Lagrange, that a particle of air agitated by two sounds, acquires a motion differing from that, which each of them would give to it separately : at the same time I must withhold my assent to the sequel, if I understand it rightly ; viz. that the sounds coalesce in consequence of this motion, until my arguments on this subject have been refuted. While this remains unattempted, the prolongation of the present controversy promises to be of no advantage to science : I will therefore in future disregard all remarks and objections which do not attack the fundamental principle of my theory.

JOHN GOUGH.

*Middleham near Kendal, Feb. 11, 1803.*

### III

*Observations on the two lately discovered celestial Bodies. By*  
WILLIAM HERSCHEL, LL. D. F. R. S.

(Concluded from Page 128.)

WITH regard to the fifth, concerning satellites, it may not be easy to prove a negative ; though even that, as far as it can be done, has been shewn. But the retention of a satellite in its orbit, it is well known, requires a proper mass of matter in the central body, which it is evident these stars do not contain.

The sixth article seems to exclude these stars from the condition of planets. The small comas which they shew, give them

them so far the resemblance of comets, that in this respect we should be rather inclined to rank them in that order, did other circumstances permit us to assent to this idea.

In the seventh article, they are again unlike planets; for it appears, that their orbits are too near each other to agree with the general harmony that takes place among the rest; perhaps one of them might be brought in, to fill up a seeming vacancy between Mars and Jupiter. There is a certain regularity in the arrangement of planetary orbits, which has been pointed out by a very intelligent astronomer, so long ago as the year 1772; but this, by the admission of the two new stars into the order of planets, would be completely overturned; whereas, if they are of a different species, it may still remain established.

As we have now sufficiently shewn that our new stars cannot be called planets, we proceed to compare them also with the other proposed species of celestial bodies, namely, comets. The criteria by which we have hitherto distinguished these from planets, may be enumerated as follows.

- |   |                       |
|---|-----------------------|
| 1. They are celestial bodies, generally of a very small size, though how far this may be limited, is yet unknown.   | Criteria of comets.   |
| 2. They move in very excentric ellipses, or apparently parabolic arches, round the sun.                             | 1. They are small.    |
| 3. The planes of their motion admit of the greatest variety in their situation.                                     | 2. Orbits excentric.  |
| 4. The direction of their motion also is totally undetermined.  | 3. Planes various.    |
| 5. They have atmospheres of very great extent, which shew themselves in various forms of tails, coma, haziness, &c. | 4. Direction various. |
|   | 5. Great atmospheres. |

On casting our eye over these distinguishing marks, it appears, that in the first point, relating to size, our new stars agree sufficiently well; for the magnitude of comets is not only small, but very unlimited. Mr. Pigott's comet, for instance, of the year 1781, seemed to have some kind of nucleus; though its magnitude was so ill defined, that I probably overrated it much, when, November 22, I guessed it might amount to 3 or 4" in diameter. But, even this, considering its nearness to the earth, proves it to have been very small.

That of the year 1783, also discovered by Mr. Pigott, I saw to more advantage, in the meridian, with a 20-feet reflector. It had a small nucleus, which, November 29, was coarsely estimated to be of perhaps 3" diameter. In all my other pretty numerous

Comparison of the new stars in these respects.

numerous observations of comets, it is expressly remarked, that they had none that could be seen. Besides, what I have called a nucleus, would still be far from what I now should have measured as a disk; to constitute which, a more determined outline is required.

In the second article, their motions differ much from that of comets; for, so far as we have at present an account of the orbits of these new stars, they move in ellipses which are not very excentric.

Nor are the situations of the planes of their orbits so much unlike those of the planets, that we should think it necessary to bring them under the third article of comets, which leaves them quite unlimited.

In the fourth article, relating to the direction of their motion, these stars agree with planets, rather than with comets.

The fifth article, which refers to the atmosphere of comets, seems to point out these stars as belonging to that class; it will, however, on a more particular examination, appear that the difference is far too considerable to allow us to call them comets.

Account of the  
size of tails of  
comets.

The following account of the size of the comas of the smallest comets I have observed, will shew that they are beyond comparison larger than those of our new stars.

Nov. 22, 1781. Mr. Pigott's comet had a coma of 5 or 6' in diameter.

Nov. 29, 1783. Another of Mr. Pigott's comets had a coma of 8' in diameter.

Dec. 22, 1788. My sister's comet had a coma of 5 or 6' in diameter.

Jan. 9, 1790. Another of her comets was surrounded by haziness of 5 or 6' in diameter.

Jan. 18, 1790. Mr. Mechain's comet had a coma of 5 or 6' in diameter.

Nov. 7, 1795. My sister's comet had a coma of 5 or 6' in diameter.

Sept. 8, 1799. Mr. Stephen Lee's comet had a coma of not less than 10' in diameter, and also a small tail of 15' in length.

From these observations, which give us the dimensions of the comas of the smallest comets that have been observed with good instruments, we conclude, that the comas of these new stars, which at most amount only to a few times the diameter  
of



of the bodies to which they belong, bear no resemblance to the comas of comets, which, even when smallest, exceed theirs above a hundred times. Not to mention the extensive atmospheres, and astonishing length of the tails, of some comets that have been observed, to which these new stars have nothing in the least similar.

Since, therefore, neither the appellation of planets, nor that of comets, can with any propriety of language be given to these two stars, we ought to distinguish them by a new name, denoting a species of celestial bodies hitherto unknown to us, but which the interesting discoveries of Mr. Piazzi and Dr. Olbers have brought to light.

These new stars cannot with propriety of language be called planets or comets.

With this intention, therefore, I have endeavoured to find out a leading feature in the character of these new stars; and, as planets are distinguished from the fixed stars by their visible change of situation in the zodiac, and comets by their remarkable comas, so the quality in which these objects differ considerably from the two former species, is that they resemble small stars so much as hardly to be distinguished from them, even by very good telescopes. It is owing to this very circumstance, that they have been so long concealed from our view. From this, their asteroidal appearance, if I may use that expression, therefore, I shall take my name, and call them *Asteroids*: reserving to myself, however, the liberty of changing that name, if another, more expressive of their nature, should occur. These bodies will hold a middle rank, between the two species that were known before; so that planets, asteroids, and comets, will in future comprehend all the primary celestial bodies that either remain with, or only occasionally visit, our solar system.

The author proposes to call them *Asteroids*.

I shall now give a definition of our new astronomical term, which ought to be considerably extensive, that it may not only take in the asteroid Ceres, as well as the asteroid Pallas, but that any other asteroid which may hereafter be discovered, let its motion or situation be whatever it may, shall also be fully delineated by it. This will stand as follows.

Asteroids are celestial bodies, which move in orbits either of little or of considerable excentricity round the sun, the plane of which may be inclined to the ecliptic in any angle whatsoever. Their motion may be direct, or retrograde; and they may or may not have considerable atmospheres, very small comas, disks, or nuclei.

Definition of asteroids.

Others will soon  
be found out;

As I have given a definition which is sufficiently extensive to take in future discoveries, it may be proper to state the reasons we have for expecting that additional asteroids may probably be soon found out. From the appearance of Ceres and Pallas it is evident, that the discovery of asteroids requires a particular method of examining the heavens, which hitherto astronomers have not been in the habit of using. I have already made five reviews of the zodiac, without detecting any of these concealed objects. Had they been less resembling the small stars of the heavens, I must have discovered them. But the method which will now be put in practice, will completely obviate all difficulty arising from the asteroidical appearance of these objects; as their motion, and not their appearance, will in future be the mark to which the attention of observers will be directed.

by observing the  
relative motion  
as well as the  
appearance of  
stars.

Society for that  
purpose.

A laudable zeal has induced a set of gentlemen on the Continent, to form an association for the examination of the zodiac. I hope they will extend their attention, by degrees, to every part of the heavens; and that the honourable distinction which is justly due to the successful investigators of nature, will induce many to join in the meritorious pursuit. As the new method of observing the zodiac has already produced such interesting discoveries, we have reason to believe that a number of asteroids may remain concealed; for, how improbable it would be, that if there were but two, they should have been so near together as almost to force themselves to our notice. But a more extended consideration adds to the probability that many of them may soon be discovered. It is well known that the comas and tails of comets gradually increase in their approach to the sun, and contract again when they retire into the distant regions of space. Hence we have reason to expect, that when comets have been a considerable time in retirement, their comas may subside, if not intirely, at least sufficiently to make them assume the resemblance of stars; that is, to become asteroids, in which state we have a good chance to detect them. It is true that comets soon grow so faint, in retiring from their perihelia, that we lose sight of them; but, if their comas, which are generally of great extent, should be compressed into a space so small as the diameters of our two asteroids, we can hardly entertain a doubt but that they would again become visible with good telescopes. Now, should we see a comet in its aphelion, under the conditions here pointed out, and that there are many which may be in such situations, we have the  
greatest

Comets may be-  
come asteroids;

and be observed  
in their aphe-



greatest inducements to believe, it would be a favourable circumstance to lead us to a more perfect knowledge of the nature of comets and their orbits; for instance, the comet of the year 1770, which Mr. Lexell has shewn to have moved in an elliptical orbit, such as would make the time of its periodical return only about  $5\frac{1}{2}$  years: if this should still remain in our system, which is however doubtful, we ought to look for it under the form of an asteroid.

If these considerations should be admitted, it might be objected, that asteroids were only comets in disguise; but, if we were to allow that comets, asteroids, and even planets, might possibly be the same sort of celestial bodies under different circumstances, the necessary distinction arising from such difference, would fully authorise us to call them by different names.

It is to be hoped that time will soon throw a greater light upon this subject; for which reason, it would be premature to add any other remarks, though many extensive views relating to the solar system might certainly be hinted at.

*Additional Observations relating to the Appearances of the  
Asteroids Ceres and Pallas.*

Additional observations.

May 4, 12<sup>h</sup> 40'. 10-foot reflector; power 516 $\frac{1}{2}$ . I compared Ceres with two fixed stars, which, in the finder, appeared to be of very nearly the same magnitude with the asteroid, and found that its coma exceeds their aberration but in a very small degree.

The comas do not much exceed those of the fixed stars.

12<sup>h</sup> 50'. 20-foot reflector; power 477. I viewed Ceres, in order to compare its appearance with regard to haziness, aberration, atmosphere, or coma, whatever we may call it, to the same phenomena of the fixed stars; and found that the coma of the asteroid did not much exceed that of the stars.

I also found, that even the fixed stars differ considerably in this respect among themselves. The smaller they are, the larger in proportion will the attendant haziness shew itself. A star that is scarcely perceptible, becomes a small nebulosity.

10-foot reflector. 13<sup>h</sup> 10'. I compared the appearance of Pallas with two equal fixed stars; and found that the coma of this asteroid but very little exceeds the aberration of the stars.

14<sup>h</sup> 5', 20-foot reflector. I viewed Pallas; and, with a magnifying power of 477, its disk was visible. The coma of this asteroid is a little stronger than that which fixed stars of the same size generally have.



## IV.

*On the oblique Refraction of Iceland Crystal.* By WILLIAM HYDE WOLLASTON, M. D. F. R. S. *From the Phil. Trans. for 1802.*

IN the preceding communication \*, I have inserted two different measures of refractive powers, distinctly observable in the Iceland crystal, as well as an estimate of its dispersive power; but have reserved for a separate treatise, some remarks which the same mode of investigation has enabled me to make on its oblique refraction.

Iceland crystal  
well examined  
by Huygens,

The optical properties of this body have been so amply described by Huygens in his *Traité de la Lumière*, that it could answer little purpose to attempt to make any addition to those which he has enumerated. But, as the law to which he has reduced the oblique refractions occasioned by it, could not be verified by former methods of measurement, without considerable difficulty, it may be worth while to offer a new and easy proof of the justness of his conclusions. For, since the theory by which he was guided in his inquiries, affords (as has lately been shown by Dr. Young †) a simple explanation of several phenomena not yet accounted for by any other hypothesis, it must be admitted that it is entitled to a higher degree of consideration than it has in general received.

His theory,

that light is propagated by vibrations of a medium; undulating spherically in most cases; but in an oblate spheroid, in Iceland crystal:

According to that hypothesis, light proceeding from any luminous centre, is propagated by vibrations of a medium highly elastic, that pervades all space. In ordinary cases, the incipient undulations are of a spherical form; but, in the Iceland crystal, light appeared to Huygens to proceed as if the undulations were portions of an oblate spheroid, of which the axis is parallel to the short diagonal of an equilateral piece of the crystal and its centre the point of incidence of the ray.

Whence the law of this refraction: viz. that the sine of incidence has a constant ratio to the ordinate of refraction in the spheroid,

From this spheroidical form of the undulations, he deduces the obliquity of refraction; and lays down a law, observable in all refractions, at any surface of the spar, whether natural or artificial, which bears the closest analogy to that which obtains universally at other refracting surfaces; for as, in other

\* See our Journal IV. 89.

† Bakerian Lecture. Phil. Trans. for 1801.

cases,

cases, the ratio is given between the sine of incidence and sine of refraction, (or ordinate of the *spherical* undulation propagated,) so, in the Iceland crystal, the ratio between the sine of incidence and ordinate of refraction (in any one section of the *spheroidal* undulation) is a given ratio.

If ABD (Fig. 1, Plate VIII.) be any surface of the spar, let FHOK be a section of the spheroid through its centre C, and RC any ray of light falling on that surface; draw FO a diameter of the spheroid, in the plane of incidence RVO, and CT, its semiconjugate diameter, in the plane of refraction FTO. Then, if CI be the refracted ray, VR, the sine of incidence, shall be to EI, the ordinate of refraction parallel to FC, in the constant ratio of a given line N to the semidiameter FC.

Illustration by diagram.

In any other plane of incidence, the ratio of sine to ordinate is also constant; but it is a different ratio, according to the magnitude of that diameter in which the plane of incidence intersects the ellipse FHOK.

But the refraction will differ, in different planes of incidence, as the diameter of the ellipse, &c.

When the incidence of the ray passing from any medium of greater density upon the surface of this spar, is such that the emergent ray becomes parallel to the surface, the ordinate of refraction is then a semidiameter of the spheroid; and, accordingly, the refractive power of this spar, when examined by means of a prism in different directions, should be found to vary as that semidiameter which coincides with the plane of incidence and refracting surface.

as may be seen by the angle of total reflection.

The observations that I have made on this substance, accord throughout with this hypothesis of Huygens; the measures that I have taken correspond more nearly than could well happen to a false theory, and are the more to be depended on, as all my experiments, excepting the last, were made prior to my acquaintance with the theory, and their agreement was deduced by subsequent computation.

Hypothesis of Huygens confirmed.

*Exp. 1.* The oblique refraction of this spar is rendered visible by cementing a surface of it to a prism of flint-glass, with a little balsam of Tolu. When the line of sight befalls an acute angle of a natural surface of the spar, the refractive power is seen to be less than in any other direction, and may be expressed by the sine 1,488, or its reciprocal 0,67204.

Experiments on the oblique refraction in different planes.

*Exp. 2.* When the plane of incidence is parallel to one of the sides, the power is 1,518, of which the reciprocal is 0,6587.

*Exp.*



*Exp. 3.* In a direction at right angles with either side, it is found still higher, being 1,537, or its reciprocal 0,6506.

*Exp. 4.* And in the plane bisecting an obtuse angle, the refractive power of the natural surface appears greater, and is expressed by the sine 1,571, or its reciprocal 0,6365.

and surfaces.

*Exp. 5.* When either of the two greatest solid angles of the spar contained under three obtuse angles, is cut off by a polished surface making equal angles with each of its sides, the same refractive power 1,488 is found in all directions. By the theory also, the section of the spheroid is in this case a circle, and every semidiameter (FC) the same, since the plane is at right angles to the minor axis.

*Exp. 6.* If a plane surface be formed bisecting an obtuse angle of the spar, and applied to a prism, the same minimum of refraction 1,488, is found in a direction that coincides with the preceding plane, and therefore with the major axis of the generating ellipse; but, as the direction is varied, it increases so rapidly as soon to exceed the power of glass, and to be no longer ascertainable by the angle of incipient reflection.

Regular refraction.

*Exp. 7.* The regular refraction of this spar is also too great for examination by means of any prism, for want of a medium of union of sufficient density; but, by trial in the usual method, it measured, on an average of several experiments, 1,657, or its reciprocal 0,6035.

Spheroid which regulates the refractions.

By assuming, as Huygens has done, the equality of this power with the maximum of the oblique refraction, we have sufficient data for construction of the spheroid by which the refractions are regulated; for we have 0,67204 (*Exp. 1.*) as major axis of the generating ellipse, and 0,6035 (*Exp. 7.*) will be the minor axis, parallel in position to the short axis of the spar.

The angle of inclination of this axis to the surfaces of the spar, if supposed to be equilateral, may be computed by spherical trigonometry, from any other angle that has been ascertained by measurement.

The measures that I have taken are not exactly those of Huygens; but I nevertheless hold them in equal estimation, from the conformity which I find they bear to each other, by assistance of his theory.

Deduction of its dimensions and position.

*Exp. 8.* I measured with care an angle at which two surfaces of the spar are inclined to each other, and found it to be  $105^{\circ} 5'$ .

Hence



Hence, the greater angle of the surfaces themselves may be computed to be  $101^{\circ} 55'$ ; and the angle which the short axis makes with each plane surface is  $45^{\circ} 23' 25''$ .

If GSMP (Fig. 2.) be a plane bisecting an obtuse angle of the spar, the section of the spheroid in that plane passes through the axis CS, and therefore is the generating ellipse. By calculating from the known dimensions of its major axis CP 0,67204, its minor axis CS 0,6035, and the angle GCS =  $45^{\circ} 23' 25''$ , CG will be found \* to be 0,6365, of which the reciprocal is 1,5736, differing but little from 1,571, as it appeared by measurement. (*Exp.* 4.)

Again, if ABDE (Fig. 4.) be one of the natural surfaces, and PGp the ellipse formed by that section of the spheroid, PC being as before 0,67204, and CG 0,6365, the reciprocal of 1,571 found by measurement, (*Exp.* 4) then the semidiameter CT, parallel to the side AE, which makes an angle TCP  $39^{\circ} 2\frac{1}{2}'$ , will be found to be 0,6573, instead of 0,6587, and its reciprocal 1,5215, instead of 1,518. (*Exp.* 2.)

The semidiameter also, in the direction of CL, perpendicular to the side, at an angle LCP  $50^{\circ} 57\frac{1}{2}'$ , is found by calculation 0,650, and its reciprocal 1,539, instead of 0,6506 and 1,537. (*Exp.* 3.)

From the foregoing data, the course of a ray perpendicular to the surface of the spar may likewise be computed; for, since the sine of incidence is then nothing, the ordinate of refraction must be also nothing, and the ray will be refracted along the semiconjugate diameter CM. (Fig. 2.)

By calculation †, the angle which this conjugate makes with the perpendicular is  $6^{\circ} 7\frac{1}{2}'$ . But, by the following measurement, it appears to be  $6^{\circ} 16'$ .

*Exp.* 9. A piece of spar that measured 1,145 inch in thickness, was laid upon a line, and showed two images that were removed from each other  $\frac{126}{1000}$  of an inch. Then, as 1,145 : 0,126 :: radius : tang. of  $6^{\circ} 16'$ .

The different results deduced from theory and from observation, will be seen at one view in the following statement:

\* (Fig. 3.) CS : CP :: tang. PCG : tang. PCp sec. PCp : sec. PCG :: CP : CG; Comparative statement of results from observation and from theory.

† (Fig. 5.) CS : CP :: tang. PCG : tang. pCO or co-tang. PCQ; then CP : CS :: tang. PCQ : tang. PCM; and LCP = PCM = MCL.

In

In Exp. 2d,	observed	1,518;	calculated	1,5215.
3d,	——	1,537	——	1,539.
4th,	——	1,571	——	1,5736.
6th,	angle observed	6° 16'.	—	— 6° 7½'.

The angle observed differs from that obtained by computation, in a greater degree than any of the former measures; but, when the difficulty of measuring this angle with accuracy is considered, and also the greater effect of any incorrectness in the data from which a semiconjugate is computed, I think the result of this, as well of the preceding comparisons, must be admitted to be highly favourable to the Huygenian theory; and, although the existence of two refractions at the same time, in the same substance, be not well accounted for, and still less their interchange with each other, when a ray of light is made to pass through a second piece of spar situated transversely to the first, yet the oblique refraction, when considered alone, seems nearly as well explained as any other optical phenomenon.

Remarks.

## V.

*The Theory of Compound Sounds, By Mr. JOHN GOUGH\*.*

Dr. Smith's theory of compound sounds; that the pulses do not obstruct or become confounded with each other.

DR. SMITH, author of the work on Harmonics, takes for granted in his theory of compound sounds, that the pulses which proceed at the same time from a number of sounding bodies, do not clash, or obstruct one another, in their passage through the air. According to this hypothesis, each set, of any number of cotemporary sets of pulses, strikes the ear without being confounded with the rest; in consequence of which, any number of sounds may be distinctly perceived at the same time. On this supposition, a compound sound is a sensation rendered variable by the irregular manner in which the pulses of the constituent sounds succeed to one another. For, if the intervals of time between two successive pulses of

\* Manchester Memoirs, v. 653. This treatise which was not inserted at an earlier period in our Journal, on account of the pressure of other matter, has become of still more interest as containing the theory to which the author's late discussion with Dr. Young is directed.—W. N.

one of the constituent sounds be not equal to the same intervals belonging to the sound or sounds which accompany it, the secondary intervals, or small parts of time separating the pulses which fall in succession on the ear, will vary in magnitude; in the same manner that the distances between the figures upon the face of a barometer and its nonius vary, none on the slider coinciding with those on the fixed plate excepting the highest and lowest. I have chosen this familiar instrument to illustrate Dr. Smith's method of explaining the physical cause of compound sounds, because it affords a visible example of a cycle of pulses, according to his notion of the subject.

The intervals of the pulses are not equal, but increase and diminish periodically, as in a scale and nonius.

The sketch which I have exhibited of Dr. Smith's hypothesis shews, that he allowed that a number of simple sounds might exist in concert, and strike the ear in a distinct manner, without suffering any interruption in their motions from the interference of their pulses. But a late writer on sound rejects this axiom in Harmonics as a mathematical inconsistency; and substitutes the following theory of compound sounds in the room of it. If two musical strings, differing in their times of vibration, happen to vibrate in concert, they do not occasion two distinct sounds in the opinion of this gentleman, because the strings agitate the air in conjunction; consequently the pulses, which one of them would actually form in an undisturbed atmosphere, must unavoidably clash with those which the other string would produce in similar circumstances. Hence the waves of air belonging to both strings are interrupted in their natural progress, and are compelled by their mutual interference to coalesce, thereby producing a new succession of pulses, constituting a single sound in the place of two. This sound is of a peculiar kind; for, the pulses which it consists, are separated by unequal intervals of time, and disposed in cycles.

Another theory (Dr. Young's),

that the pulses coalesce;

and form a peculiar sound, having its pulses unequal and disposed in cycles.

The merit of the preceding theory, when compared with Dr. Smith's hypothesis, must be ascertained by contrasting it with a variety of facts, which are furnished by the phenomena of compound sounds, and make a part of every man's experience. For, if it be found upon examination to be repugnant to these facts, it will prove inconsistent with nature, and cannot fail of disappointing the inventor's expectations.

Examination of the latter theory.

Were it possible for a number of sounds to coalesce, and form but one, the compound would acquire sensible properties peculiar

If sounds could coalesce they would form a new compound;



in which the ear could not distinguish the component parts.

Chemical comparison.

Compound sounds are mixtures; not combinations.

Instances. The flute and violin perfectly distinguishable in concert,

the courses of the two sounds are distinguished when they differ.

to itself, and at the same time lose the distinguishing characters of its elements, some of which are incompatible with the qualities of an individual. On this supposition, the presence of the constituent sounds could not be detected by the ear in this newly created being: on the contrary, an experimental process would be required to analyse every compound sound the first time it attracted a man's attention, for the same reason that a chemist finds it necessary to analyse a substance with which he is unacquainted. The abstract term *coalescence* is used, in a physical sense, to signify any intimate union of bodies or the powers of bodies; and the introduction of the term into language proves the existence of the principle in nature, or more properly in the human mind. For, when a number of agents act in conjunction upon one of the senses, we have two ways of conceiving their mode of operation. If the sensible effects of each agent be distinctly perceived, we attribute a separate action to every member of the assemblage, and call the aggregate a mixture: this is the conclusion of a person who tastes an infusion of pepper in vinegar. On the other hand, when we know that certain agents are present without being able to recognize their distinguishing powers, in the room of which we find qualities of a different description, we pronounce the aggregate to be in a state of coalescence. This is the situation of the chemist, who tastes common salt, but cannot perceive the presence of soda and the muriatic acid. It is my business then to prove compound sounds to be *mixtures*, not aggregates by coalescence. This I shall endeavour to do, by shewing that they have properties which belong not to individuals, such as a number of tones, a variety of directions, and several sets of pulses.

First, the tones of a flute and violin are as distinct to sense as any two things can be when they are sounded separately; and I appeal to common experience to determine, if they are not equally distinct when heard in concert. Taking it for granted that the answer will be in the affirmative, I pronounce the aggregate to be a mixture of sounds in one case. Secondly, if a violin sound in front of the hearer, and a flute be heard at the same time in an oblique situation, the person thus circumstanced is able to determine the relative positions of the two instruments, which shews the aggregate to have two co-temporary directions. It is therefore a mixture of sounds, not a single

a single sound. Thirdly, I have found by making the experiment, that any number of musical strings may be made to vibrate by a compound sound acting upon them, provided this compound be occasioned by an equal number of strings with the former, having one in the latter set in unison with each one in the preceding set. This is an experimental proof that there are as many sets of pulses in an aggregate of sounds as that aggregate contains elements, because no string whatever is in unison with a concord or discord. Lastly, if it were possible for sounds to coalesce, men would never hear any thing more than one noise at one time: The general hum would have varied perpetually from the extinction of existing sounds; and the intrusion of fresh ones; but the human mind would have had no conception of two cotemporary sounds; because the ear being in that case incapable of conveying the complex sensation, the idea of such an existence would have transgressed the sphere of human knowledge. The preceding arguments are drawn, for the most part, from common experience; and they shew, that the free passage of cotemporary sounds through the air may be safely admitted as an axiom in harmonies. I shall therefore proceed to prove the same proposition to be consistent with the doctrine of forces.

A number of strings will be set into vibration by the compound sound of other strings giving unison to each respectively.

If sounds coalesce we should constantly hear one varying noise.

The propagation of sound through the atmosphere, and the nature of aëreal pulses are commonly explained in elementary books of natural philosophy; I shall, for this reason, enumerate only a few particulars, the recollection of which will be found useful.

Mechanical arguments.

Proposition I. Two contiguous particles of air which are agitated by a vibrating body, either directly or by the intervention of an elastic medium, receive two motions from each impulse; *first*, an absolute motion carries them to a greater distance from the sounding body, and afterwards brings them towards it again, both the progress and regress being performed in the time of a single vibration: *second*, a relative motion resulting from the former, compels the two particles to approach and recede alternately, which double motion is also accomplished in the time of a single vibration.

Prop. 1. Vibration of the particles of air, in sound.

Proposition II. Both the absolute and relative motions are the greatest amongst those particles which are nearest the sounding body, and they diminish as the distance from that body increases; but, in all cases, the change of its place too small to be perceived

Prop. 2. The particles are not sensibly removed.



perceived by the ear, on which account every particle preserves a fixed position in respect of this organ and its connections.

For each corpuscle is confined within the circumference of a physical right line, the diameter of which is determined by its own absolute motion.

*Physical right line.*

Prop. 3. A particle in the intersection of two rows of particles conveying different sounds, will be urged in those lines by forces varying in their ratio.

Proposition III. If two sounding bodies, affording different notes, act in conjunction upon the same particle, through the media of two right lines of similar particles connecting them with it, this particle will be urged at the same instant in the direction of these lines, by two forces having an inconstant ratio.

For, let the particle A be urged, by the acuter sound, in the line SA, and by the graver, in the line TA; (*vide* Fig. 2, Plate IX.); then the contiguous particle V, placed in SA, will approach to, and recede from A more frequently than W, similarly placed in TA, by Prop. I. : consequently the force of V upon A will vary in a quicker manner than the force of W upon A; but this variation of ratio is limited in time; because it evidently begins and ends with the cycle of the vibrations of the sounding bodies.

Prop. 4. The right lined motion of the particle necessary to form a sound by coalescence is impossible.

Proposition IV. The coalescence of two sounds is impossible on mechanical principles.

For, suppose the thing possible; then the coalescence of two sounds requires, that a particle of air should possess a motion, compounded of the motions which the two sounding bodies would impart to it separately; and that this compound motion should act in a given right line, for an assignable part of time, otherwise it could not excite a similar motion in the elastic particles occupying that given right line. Let A be such a particle, and let the construction used in the last proposition, be retained; consequently, (*Principia*, Prop. 23, Lib. 2.) VA and AW are in the ratio of the forces that act at any moment in the right lines TA and SA. Make AK as AW, and draw KL parallel to AW, and make it as AV; also join AL; then will the particle A be urged in the direction LA at that instant. But the ratio of AK to KL varies perpetually, by Prop. III.; therefore the species of the triangle AKL is equally inconstant; consequently the compound force does not act in a given direction for an assignable part of time. Now the production and propagation of motion in a given right line requires force

to



to be combined with time, which combination is wanting in the present instance; wherefore the coalescence of sounds is impossible.

Proposition V. It may be demonstrated from mechanical principles, that a number of distinct cotemporary sounds cannot do otherwise than produce distinct sensations.

Prop. 5. Distinct cotemporary sounds produce distinct sensations.

In order to make the necessary diagram as simple as possible, let the directions of two cotemporary sets of pulses be represented by the right lines SM and TN, lying in the same horizontal plane, and intersecting in the point A; also, let BCD be the horizontal section of the hearer's head, made by the same plane; and suppose the centre of the axis of hearing to be at O; draw OM, ON perpendicular to SM, TN. Now I have shewn in the preceding paper\*, that if a set of pulses move in either of the right lines SM, TN, it will excite a sensation in that part of the head which is cut off by a vertical plane, passing through one of the perpendiculars OM, ON. It also appears from the last proposition, that the impulses of the vibrating bodies, acting in the lines SA, TA, do not compel the particle A to move in any given intermediate direction, as LA. But, according to the second proposition, the position of the particle A, is fixed in respect of the planes MO, NO; that is, though the corpuscle actually changes place, in respect of the geometrical point A, it is always found in the intersection of the physical right lines SM, TN. Now the two vibrating bodies continue to act in the directions of these right lines, consequently the particle A is constantly urged in these lines by two forces, which, though variable in magnitude, are combined with time; which circumstance enables the corpuscle to transmit the impulses of one body to M, and those of the other to N. What has been demonstrated of the particle A, may be affirmed of any other particle, which is the intersection of two right lines parallel to SM, TN; in other words, it may be affirmed of two sets of pulses; and the same demonstration may be extended to three sets, &c.

Corollary 1. The substance of this and the preceding proposition will apply to all elastic mediums; hence it happens, that a plate of glass, &c. in a state of vibration, will conduct a foreign sound, whilst it produces one of its own; for the

These propositions apply to sounds conducted through solids, &c.

\* Manchester Mem. v. 342, or Philos. Journal, II. 460.

same reason, if light be considered as a vibrating medium, one particle of the luminous fluid may assist in transmitting two sensations.

The directions  
will not be per-  
ceived unless  
they differ by a  
certain angle.

*Corollary 2.* When the inclination of the planes MO, NO, is less than a given angle, the ear cannot distinguish the relative positions of the sounding bodies; it therefore refers them to the same place.

Interval of  
sound.

Analysis of the  
human voice.

The first time I perused Dr. Smith's *Harmonics*, Dr. Young's objection occurred to me; but the preceding train of arguments removed the scruple, without discovering the author's reasons for treating this article of his work with so much brevity. Perhaps the demonstration, which cost me an effort of study, was an intuitive conclusion in his comprehensive mind. As soon as the proposition was established, I assented to his definition of an interval of sound, allowing it to be a quantity of a certain kind, terminated by a graver and an acuter sound. The demonstration of Prop. V. convinced me, that intervals of this sort may be subdivided by the interposition of one or more intermediate sounds, which concession formed the basis of my analysis of the human voice.\* Speculative men may differ in opinion about the origin of the small intervals which form the tones of various voices; but they must exist, whether we ascribe them to an undulating motion like that of a stretched cord, or to the cotemporary vibrations of a system of elastic bodies. It does not appear, that Dr. Young was acquainted with my paper at the time he composed his own; but he found it necessary to allow the tone of the larynx to receive various modifications from the vibrations of the adjacent parts. His theory therefore differs from mine in this particular only: he pronounces the voice to be a compound by coalescence; I deny the possibility of such a compound, and call it a mixture of imperfect unisons. This mixture appears to be a single sound, because it has but one direction; for the proximity of the various parts contributing to the formation of it, disqualifies the ear, so that it cannot perceive their relative positions, and compels it to refer them all to one place, by Corollary 2 to proposition V.

\* Manchester Mem. v. 58, or Phil. Journal, Quarto, IV. 46. for an abridged statement.

A certain

A certain class of sounds, which, for the sake of brevity, were not noticed in my paper on the voice, deserve a place in the present communication. If a finely-toothed file pass slowly over a smooth elastic substance, such as a piece of horn, it makes a grating noise; but if the velocity of the instrument be sufficiently increased, a continued sound is produced, which becomes more or less acute, by giving a quicker or slower motion to the file. The grating noise is occasioned by a succession of short interrupted sounds, resulting from the united vibrations of the file and the body it scratches; but the quick succession of these sounds, caused by an increase of velocity, gives rise to a secondary sound resembling the harmonical notes, being produced by a like cause. Now this sound becomes a primary object with the ear, in all probability because the pitch of it may be varied; for the first sounds proceeding from the action of the file, evidently supply nothing but the tone. Many instances of the kind occur in art and nature: the notes of all reed-instruments are of this description, and the voice must be referred to the same class, because the larynx resembles a reed-instrument in structure.

Class of sounds produced by the quick succession of other sounds; the first giving the tune or pitch, and the latter the tone or character.

## VI.

*Experiments and Observations to determine whether the Quantity of Rain and Dew is equal to the Quantity of Water carried off by the Rivers and raised by Evaporation; with an Enquiry into the Origin of Springs. By Mr. JOHN DALTON \*.*

IT is scarcely possible to contemplate without admiration the beautiful system of nature by which the surface of the earth is continually supplied with water, and that unceasing circulation of a fluid so essentially necessary to the very being of the animal and vegetable kingdoms takes place. Naturalists, however, are not unanimous in their opinions whether the rain that falls is sufficient to supply the demands of springs and rivers, and to afford the earth besides such a large portion for evaporation as it is well known is raised daily. To ascertain this point is an object of importance to the science of agriculture, and to every concern in which the procuration and

Interesting system by which the globe is supplied with water.

Whether the rain be equal to all the demands of rivers, &c.

\* Manchester Memoirs, V. 346.



management of water makes a part, whether for domestic purposes or for the arts and manufactures.

For the sake of perspicuity I have distributed the subject under four heads :

Division of the subject.

1. Of the quantity of rain and dew.
2. Of the quantity of water that flows into the sea.
3. Of the quantity of water raised by evaporation.
4. of the origin of springs.

## SECTION I.

### *An Estimate of the Quantity of Rain and Dew that falls in England and Wales in a Year.*

Annual quantity of rain and dew in England and Wales.

Inland counties have less rain than coasts; and mountainous districts have most.

Rain-gages have been fixed of late years in almost every part of the kingdom; by means of them we are enabled to determine, with considerable exactness, the depth of water that the rain yields in any given place. Inland counties have less rain than maritime ones, especially those which border on the western seas. But a still greater difference seems to take place between a mountainous country and a champaigne, or flat country: in the former there often falls double or triple the quantity of rain in a year, that there does in the latter, and never less than an equal quantity. It may be observed, that several years account of the rain at any place is required before a medium yearly quantity can be obtained with sufficient accuracy. The following is perhaps the largest collection of accounts of rain fallen in different places in England that has hitherto appeared: They are mostly taken from the Transactions of the Royal and other Societies.

Quantities tabulated.	Counties (maritime)	Places.	Mean annual depth in inches.
	CUMBERLAND - -	Keswick, 7 years	- - - 67. 5
		Carlisle, 1 year	- - - 20. 2
	WESTMORLAND - -	Kendal, 11 years	- - - 59. 8
		Fell-foot, 3 years	- - - 55. 7
		Waith Sutton, 5 years	- - - 46
	LANCASHIRE - -	Lancaster, 10 years	- - - 45
		Liverpool, 18 years	- - - 34. 4
		Manchester, 9 years	- - - 33
		Townley	- - - 41
		Crawshawbooth, near Haslingden, 2 years	- - - 60
		GLoucestershire	

<i>Counties (maritime)</i>	<i>Places.</i>	<i>Mean annual depth in inches.</i>
GLOUCESTERSHIRE	Bristol, 3 years	29. 2
SOMERSETSHIRE	Bridgewater, 8 years	29. 3
CORNWALL	Ludgvan, near Mount's Bay, 5 years	41
	Another place, 1 year	29. 9
DEVONSHIRE	Plymouth, 2 years	46. 5
HAMPSHIRE	Selbourne, 9 years	37. 2
	Fyfield, 7 years	25. 9
KENT	Dover, 5 years	37. 5
ESSEX	Upminster,	19. 5
NORFOLK	Norwich, 13 years	25. 5
YORKSHIRE	Barrowby, near Leeds, 6 years	27. 5
	Garfdale, near Sedbergh, 3 y.	52. 3
NORTHUMBERLAND	Widdrington, 1 year	21. 2

<i>Counties (inland)</i>	<i>Places.</i>	<i>Means.</i>
MIDDLESEX	London, 7 years	23
SURREY	South Lambeth, 9 years	22. 7
HERTFORDSHIRE	Near Ware, 5 years	25
HUNTINGDONSHIRE	Kimbolton, 7 years	25
DERBYSHIRE	Chatsworth, 15 years	27. 8
RUTLANDSHIRE	Lyndon, 21 years	24. 3
NORTHAMPTONSHIRE	Near Oundle, 14 years	23

General Mean - - - 35. 2

This general mean of 35. 2 inches is, I apprehend, a little above the medium for England and Wales, as the greater number of places are those where much rain falls. If we take a mean for each of the above-mentioned counties (where more than one place in a county is given) and then a general mean from the counties, the result is a reduced mean of 31. 3. Even then it may be objected that the greater part of the counties are maritime; but it must be observed, that there is no account of rain in Wales; and we may safely conclude, that the rain in Wales would exceed the last-mentioned mean as

much as the inland counties of England, not in the above list, would fall short; because Wales is both a mountainous country, and exposed to the sea.

Corrected is 31  
inches for Eng-  
land and Wales.

We will therefore conclude, that the mean annual depth of rain in England and Wales, deduced from these 20 counties, is 31 inches: A quantity which subsequent observations, I am confident, will not diminish, and probably not increase much\*.

Quantity of dew.  
It is water depo-  
sited by the cold  
of night.

It remains to estimate the quantity of dew that falls in a year. Some have doubted whether dew is derived from the air or the earth; but a proper attention to the phenomena will satisfy us, that it is a deposition of water, evaporated during the heat of the day. With respect to the quantity that falls in a year, we are much at a loss, as no daily observations have been made for a series of time that I know of: indeed, it would be difficult to prescribe a mode of observation. Dr.

Estimate by Dr.  
Hales.

Hales† relates some experiments made to determine the quantity of dew that falls upon moist earth, from which he estimates the annual dew at 3.28 inches. But it is probable that the dew which is deposited on grass is much more copious than what falls on moist earth, because grass exposes much more surface in a given acre of ground. If we take the dew at five inches annually, it will probably not be much over-rated: supposing it should be over-rated, the excess may stand against the rain that is lost by evaporation from the surface of the rain-gage each time it rains‡. Wherefore, upon the whole, we shall

Annual quantity  
of dew taken at  
five inches.

\* The editors of the Encyclopedia, under the article Weather, from 16 places of observation, make the annual mean for Great Britain 32.53 inches; and M. Cotte, in the Journal de Physique for 1791, gives a mean derived from 147 places in different parts of the world equal to 34.7 inches.

† Veg. Statics, Vol. I. page 52.

‡ Since writing the above paragraph on dew, I have had occasion to make several experiments on the subject of aqueous vapour, as it exists in the atmosphere, the result of which will, I am persuaded, materially illustrate this important question in physics.—At present I shall only observe, that the following conclusions seem deducible from the experiments above referred to.

Generalities  
concerning aque-  
ous vapor.

1. That aqueous vapour is an elastic fluid *sui generis*, diffusible in the atmosphere, but forming no chemical combination with it.

2. That temperature alone limits the *maximum* of vapour in the atmosphere.

3. That



shall have 36 inches of water at a medium annually on the surface of the earth in England and Wales, reckoning 31 for rain and five for dew. Annual quantity of rain and dew 36 inches.

According to Guthrie, the area of England and Wales is 46.450 square miles. This reduced to square feet, gives 1.378.586.880.000: which, multiplied by three feet the annual depth of rain and dew, gives 4.135.760.690.000 cubic feet of water = 153.176.320.000 cubic yards, or 28 cubic miles = 115 thousand millions of tons in weight; nearly. We must now consider how this enormous quantity of water is disposed of. Computed quantity of water that falls in a year 28 cubic miles, or 115 thousand million of tons.

There are two principal ways by which the water derived from rain is carried off again: one part of it runs off immediately into rivulets, or sinks into the earth a small way, breaks out again in lower ground in the form of springs, thence makes its way to some river, by which it is conveyed into the sea—another part is raised into the atmosphere by evaporation. We take no notice here of the decomposition of water by vegetables; because it is presumed that in the course of nature the principles are combined and water formed again. It is carried off by rivers, by springs, and by evaporation.

3. That there exists at all times, and in all places, a quantity of aqueous vapour in the atmosphere, variable according to circumstances.

4. That whatever quantity of aqueous vapour may exist in the atmosphere at any time, a certain temperature may be found, below which a portion of that vapour would unavoidably fall or be deposited in the form of rain or dew, but above which no such diminution could take place, chemical agency apart. This point may be called the *extreme temperature* of vapour of that density.

5. And that whenever any body colder than the extreme temperature of the existing vapour is situated in the atmosphere, dew is deposited upon it, the quantity of which varies as the surface of the body and the degree of cold below the extreme temperature.

N. B. The *extreme temperature* of vapour in the atmosphere varies all the way from the *actual* temperature of the atmosphere to 10, 15, 20 or more degrees below it.—The point may generally be found in the hottest months by pouring cold spring water into a dry and clean glass, and marking what degree of cold is sufficient to produce a dew on the outside of the glass; at other times frigorific saline solutions may be used.

## SECTION 2.

*An Estimate of the Quantity of Water that flows into the Sea from England and Wales in a Year.*

Method of determining how much water flows through a river.

To calculate the quantity of water that flows down any one river into the sea in a given time, seems at first view a question of great difficulty. The necessary data, however, may be obtained with considerable exactness, by proper observations, and then it becomes an easy case of mensuration. Dr. Hutton, in his Philos. and Mathemat. Dictionary, article River, proposes a very good method to determine by experiment the velocity of a river:—A cylindrical piece of light wood, its length somewhat less than the depth of the waters, is to be taken, and a few small weights attached to one end in order to make it swim upright. To the other end a small rod is fixed in the centre in direction of the axis. This being suffered to float down the stream will move with the velocity of the water; and if the rod be observed to incline towards the river upward or downward, it shews the current to be more rapid at the bottom or surface respectively.

This experiment being made in the middle and near the sides of a river, a medium velocity may be obtained. Then the medium, breadth, depth, and space run over in a certain time being multiplied together, will give the quantity of water that flows down in that time.

Estimate of water delivered by the Thames.

Dr. Halley, in order to estimate the quantity of water that flows into the Mediterranean sea by means of rivers, makes a comparison of the great rivers of Italy, &c. with that of the Thames. (Philos. Transact. Abridg. Vol. 2. Page 110). He assumes the breadth of the Thames at Kingston Bridge to be 100 yards, its depth three yards, and velocity two miles per hour. He professedly over-rates the dimensions, in order to allow more than a sufficiency for the streams received below Kingston. This assumption gives the area of a transverse section of the river = 300 square yards, and the quantity of water flowing down = 20.300 000 tons in a day. This must be over-rated by at least, I think, one third:—If the breadth be assumed 100 yards, the depth three, and velocity two miles per hour, it will then give  $\frac{2}{3}$  of the result above mentioned; or it will amount to the same thing if we take  $\frac{1}{3}$  part from all the three data assumed by Dr. Halley, the result being  $\frac{2}{3}$  of that above,



above, amounting in the year to 166.624.128.000 cubic feet, which is a little more than  $\frac{1}{23}$  part of all the rain and dew in England and Wales in a year, as above deduced.

By an inspection of the annexed map of the rivers of this country, as well as by a fair calculation, it appears, that the water of the Thames is drawn from an extent of country of about 600 square miles, or  $\frac{1}{8}$  of the area of the whole, nearly. The Severn, including the Wye, spreads over an equal or greater extent of country: and that collection of rivers which constitutes the Humber is superior to either of the other two in this respect. As far as my own observation goes, the Severn and Wye must disembody as much or more water than the Thames; the Humber I have not seen collectedly, but have noticed most of the branches constituting it, and should apprehend it cannot be inferior to the Thames: all other circumstances being the same, the quantity of water carried down by any river should be as the area of the ground from which the water is derived, and on this account the Humber ought to exceed the Thames\*.

The Severn, which is partly derived from the mountainous and the districts country of Wales, is certainly the most rapid of the three rivers, and probably carries down the most water: as the Thames, however, is generally considered to take the lead, we will suppose, upon the whole, that these three rivers are equal in this respect.

The counties of Kent, Suffex, Hampshire, Dorsetshire, which supply Devonshire, Cornwall, and Somersetshire, from the Medway them with water, to the lower Avon inclusively, in an extent of 11.000 square miles, do not present us with many large rivers. From their number and magnitude, we cannot form a high estimate of their produce. The quantity of rain for those counties is indeed near the average for the kingdom, as far as the preceding observations determine; but the milder temperature of their winters and greater heat of their springs and summers, will cause a greater evaporation than in some other parts: It is probable the rivers in these counties may amount, when taken together, to  $1\frac{1}{2}$  times the magnitude of the Thames. The rivers that disembody their waters on the coast of Lin-

\* A more perfect theorem will be given afterwards, for finding the quantity of water carried down by any river.



colnshire, Norfolk, Suffolk and Essex, from the Humber to the Thames, though drawn from a country of 7000 square miles, manifestly fall far short of the Thames. The two places in this district, for which we have accounts of the rain, Norwich and Upminster. give a mean of only  $22\frac{1}{2}$  inches annually. This, with the flatness of the country, which prevents the water from running off in some degree, makes the rivers much less than what might otherwise be expected from the extent of ground. There are but three or four of any consequence, Probably all the rivers may amount to half the size of the Thames. There remains above 6000 square miles in Wales, from the Wye to the Dee, inclusive of the last, and the northern counties of Lancaster, Westmoreland, Cumberland, Northumberland, and Durham, with part of Cheshire and a small part of Yorkshire, from the Mersey round by the Tweed to the Tees, amounting to 7 or 8000 square miles, to be estimated.

The rivers upon the whole are considered as carrying off 13 inches out of the 36 in their fall annually.

These two divisions, though not larger than some others, abound in rivers, many of which are considerable in magnitude and of great rapidity. The rains at an average, it is probable, are double what they are in the S. E. counties of the kingdom. The rivers in these two districts cannot fairly be estimated, I think, at less than *four* times the Thames. It appears, then, that by this estimation, the water carried off by all the rivers in England and Wales, may amount to *nine* times that carried off by the Thames = 13 inches of rain. There remains still *sixteen* times the water of the Thames, or 23 inches of rain to account for, before we have disposed of all the rain and dew.

### SECTION 3.

#### *An Estimate of the Quantity of Water raised by Evaporation.*

Evaporation

Upon looking over the surface of any country, three principal varieties of surface present themselves to view, as far as respects evaporation, namely, *water*, ground covered with grass and other vegetables, and bare soil. The difficulties that occur in attempts to find the quantity of water evaporated in those three cases, are perhaps the principal reason why our knowledge on this head is so imperfect.

from water, bare soil, and covered ground.

As far as experiments hitherto made authorize us to draw conclusions, it should seem that the evaporation from water is greatest;

greatest; that from green ground is probably next, and that from bare soil the least: though we may presume, that the copious dews upon the grass more than supply the excess of evaporation above what takes place from a moist uncovered soil.

The most satisfactory experiments I have seen an account of, relating to the evaporation from a surface of water, are those of Dr. Dobson, made at Liverpool, in the years 1772, 73, 74 and 75. (Vid. Philos. Transac. Vol. 67.)—He took a cylindrical vessel of 12 inches diameter, and having nearly filled it with water, exposed it besides his rain-gage of the same aperture, and by adding water to it, or taking it away occasionally, he kept the surface nearly of the same height, and carefully registered the quantities added or taken away, by a comparison of which with the rain, the amount of the evaporation was ascertained. The mean monthly evaporation for four years was—January 1.50 inches.—February 1.77;—March 2.64.—April 3.30.—May 4.34.—June 4.41.—July 5.11.—August 5.01.—September 3.18.—October 2.51.—November 1.51.—December 1.49.—In all 36.78 inches. The mean rain for the same time was 37.48 inches.—In the year 1793 I found the evaporation from water in a similar way at Kendal for 82 days in March, April, May and June to be 5.414 inches. The greatest quantity evaporated on one of the hottest and driest days in summer was a little above .2 of an inch in depth.

The experiments to determine how much is evaporated from green ground and from moist earth, are very few that have come to my knowledge. Dr. Hales, from a few experiments, calculates that moist earth only throws off  $6\frac{2}{3}$  inches annually.—This calculation must be far below the truth. Dr. Hales's estimate from moist earth.  
Dr. Watson, Bishop of Llandaff, found that in a dry season there evaporated from a grass plat that had been mowed close, about 1600 gallons in an acre per day, which amounts nearly to .07 of an inch in depth; and that after rain the evaporation was considerably more. Now supposing .07 to be the medium daily evaporation for May, June, July and August, and that as much is raised in these four months as in all the rest of the year, the annual evaporation in such circumstances will be 17 or 18 inches, which is but half that observed from water at Liverpool, and six inches less than the reserve of rain stated above. Dr. Watson from a grass plat.

Experiment on a tube of earth to shew how much water ran off the ground; how much sunk in; and how much evaporated,

In order to ascertain this point more fully, and to investigate the origin of springs, my friend Thomas Hoyle, jun. and self, practised an expedient as follows, beginning in the autumn of 1795. Having got a cylindrical vessel of tinned iron, 10 inches in diameter and three feet deep, there were inserted into it two pipes turned downwards for the water to run off into bottles: the one pipe was near the bottom of the vessel; the other was an inch from the top. The vessel was filled up for a few inches with gravel and sand, and all the rest with good fresh soil. It was then put into a hole in the ground, and the space around filled up with earth, except on one side, for the convenience of putting bottles to the two pipes; then some water was poured on to sadden the earth, and as much of it as would was suffered to run through without notice, by which the earth might be considered as saturated with water. For some weeks the soil was kept above the level of the upper pipe, but latterly it was constantly a little below it, which precluded any water running off through it. Moreover, for the first year the soil at top was bare; but for the two last years it was covered with grass the same as any green field. Things being thus circumstanced, a regular register has been kept of the quantity of rain water that ran off from the surface of the earth through the upper pipe (whilst that took place) and also of the quantity of that which sunk down through the three feet of earth, and ran out through the lower pipe. A rain-gage of the same diameter was kept close by to find the quantity of rain for any corresponding time.



The following Table shews the Result.

Tabulated results.

Water through the two Pipes.				Mean.	Mean	Mean
	Inch.	Inch.	Inch.	inch.	Rain.	Evap.
	1796.	1797.	1798.		inch.	inch.
Jan.	1.897—	,680—	1.774+	1.450+	2.458	1.008
Feb.	1.778—	,918—	1.122	1.273	1.801	,528
March	,431—	,070—	335	,279	,902	,623
April	,220—	,295—	,180	,232	1.717	1.485
May	2.027—	2.443+	,010	1.493+	4.177	2.684
June	,171—	,726—	—	,299	2.483	2.184
July	,153—	,025	—	,059	4.154	4.095
Aug.	—	—	,504	,168	3.554	3.386
Sept.	—	,976	—	,325	3.279	2.954
Oct.	—	,680	—	,227	2.899	2.672
Nov.	—	1.044	1.594	,879	2.934	2.055
Dec.	,200	3.077	1,878+	1.718+	3.202	1.484
<hr/>						
	6.877—	10.934—	7,379	8.402	33.560	25.158
Rain	30.629—	38.791—	31,259			
Evap.	23.725—	27.857—	23.862			

The following observations were made when the water Variation of the passed through both pipes: that is, when the vessel was filled experiments. up with earth above the level of the upper pipe.

	Top pipe	Bottom pipe.
	Inch.	Inch.
1796. Jan.	25— ,190—	,280
	30— ,080—	,114
Feb.	2— ,100—	,254
	8— ,196—	,140
May	1— ,163—	,000
	10— ,060—	,400
	12— ,312—	,175
	15— ,190—	,200
June	3— ,120—	,040
Total		1.411— 1.603

The column of mean evaporation is derived by taking the Remarks on the difference of the two columns preceding it; but it should be tables. observed that though this method is sufficiently exact in taking the

the year together, it is not so in taking the months severally, because it presumes that the earth in the vessel contains the same quantity of water at the end of each month, or is saturated with it; whereas in the summer months it is frequently short of saturation. The consequence is, that the evaporation appears from this table to be something less than it really is in the summer months, and something more in the autumnal \*.

Conclusions.  
Evapor. from  
ground is 30  
inches.

From these experiments it seems we may conclude—1st. That the quantity of water evaporated, *in the circumstances above related*, amounts to 25 inches of rain annually; to which if we add five inches for the dew, it will give 30 inches of water raised annually.

It increases with  
the rain.

2d. That the quantity of evaporation increases with the rain, but not proportionally. Thus, 1797 gave the most rain and the greatest evaporation, &c.

Deep bare soil  
and sod do not  
differ.

3d. That it does not appear there is much difference betwixt the evaporation from bare earth, when there is sufficient depth of soil, and that from ground covered with vegetating grass. The account in 1796 is much what might have been expected, if the earth had been covered with grass.

Whether this  
evap. of 30 and  
the rivers of 13,  
making 43 in-  
stead of 36 (the  
rain) indicate  
another supply?

As this account of evaporation, amounting to 30 inches, exceeds the medium reserve of rain of 23 inches, it demands an enquiry whether the rain is adequate, or whether the earth derives a supply of water from some subterranean reservoir, according to the opinion of some philosophers.

Reply; nega-  
tive.

With respect to the deficiency of 7 inches, there are *three* causes to be assigned for it, which appear to me fully sufficient, without having recourse to any source but that of rain for the supply of the earth in general.

The evapor.  
was taken a lit-  
tle too great.

1st. In the account of the rain that passed through the earth in our evaporating vessel, there are a few monthly products marked, + those were occasioned by the bottle that received the water through the pipe being found with the water running over; this loss was placed to the account of evaporation; it could not be much, as the water was taken several times in a month, but possibly might amount to one inch in the year.

The rain at  
Manchester ex-  
ceeds the me-  
dium;

2d. The rain at Manchester, being  $33\frac{1}{2}$  inches annually, exceeds the medium of 31 inches; and consequently, ac-

\* N. B. The earth in the vessel always appeared as well supplied with moisture as the ground around it, in the driest weather,

cording

According to the preceding observations, the evaporation ought to exceed the medium.

3d. But the principal cause of the excess in our account of evaporation, I conceive to be the prevention of the water running off from the surface of the earth at the top, by having the earth below the level of the upper pipe: it has been seen, that when the earth was above that level, a great part of the water came off that way, by which the surface was sooner dried: whereas by forcing all the water to sink through the earth or stand on its surface, a greater degree of moisture perpetually existed at the surface, and consequently afforded a greater scope for evaporation, than the surface of the earth in general would do.

Upon the whole then I think we may fairly conclude—that the rain and dew of this country are equivalent to the quantity of water carried off by evaporation and by the rivers. And as nature acts upon general laws, we ought to infer, that it must be the case in every other country, till the contrary is proved.

This conclusion being admitted, we are enabled to deduce a general theorem for the quantity of water carried down into the sea by any river in any country (on the supposition that all rivers are ramified alike) provided we have certain data: these data are the length of the river, and the excess of the rain above the evaporation in the country from which the water of the river is drawn: also, it should be known by observation, how much water some one given river carries down.

For, from the principles of geometry, the area of country from which any river is supplied, will be as the square of the length of the river; and the quantity of water carried off, will be in the compound ratio of the area of the country, and the excess of the rain and dew above the evaporation.

Thus, let  $L$  = the length of any river,  $E$  = the excess of rain and dew above the evaporation, and  $Q$  = the quantity of water disembogued in any given time by that river;  $l$  = the length of any other river,  $e$  = the excess, &c. and  $q$  = the

quantity of water; then we shall have  $q = \frac{Q l e}{L E}$ .

*Ex. gr.* Suppose the length of the Thames = 200 miles, and the excess = 5 inches, estimating the rain and dew at 30 inches and evaporation at 25; and suppose the river Kent, in Westmorland,

and the water not being suffered to run freely off at top, did increase the evaporation,

Hence the rain and dew fully supply the evaporation and the rivers.

Theorem for deducing the quantity of water carried by a river, &c.

Statement.

Example.



Westmorland, to be 20 miles in length, and the excess 35 inches, the rain and dew being supposed 65, and evaporation 30 inches.

$$\text{Then, } \frac{20^2 \times 35 \times Q}{200^2 \times 5} = \frac{7Q}{100} = q \text{ or } Q = 14\frac{2}{7}q; \text{ which}$$

result, I believe, will be found to accord nearly with the measurement of the two rivers on the principle before mentioned.

#### SECTION 4.

##### *On the Origin of Springs.*

**Cause of springs.** The origin of springs has always been justly considered as a question of natural history worthy of investigation,—In the infancy of science hypotheses are formed to account for phenomena; but when facts are discovered totally inconsistent with an hypothesis, it ought to be discarded. This does not seem to have been the case in the subject before us; for various opinions are still held by some, which it is impossible to support by facts. The object of the following remarks and experiments is to ascertain the disputed point if possible.

**Opinions.** There are *three* opinions respecting the origin of springs which it may be proper to notice.

1. Rain and dew.

1st. That they are supplied entirely by rain and dew.

2. Subterranean reservoirs.

2d. That they are principally supplied by large subterranean reservoirs of wates.

3. Filtration from the sea.

3d. That they derive their water originally from the sea, on the principle of filtration.

**Whether the first cause be insufficient.**

It is obvious, that before we pay any attention to the two latter opinions, the causes assigned in the first ought to be proved insufficient by direct experiment. M. de la Hire is the only one who has attempted to do this, as far as my information extends, in the Parisian Memoirs for 1703. He procured a leaden vessel eight feet deep, having a pipe at the bottom; this he buried in the earth, and filled with soil of sand and loam, exposing the surface to receive all the rain that fell. After 15 years trial, he found that no water had run through the pipe at the bottom.

**De la Hire's experiment.** No rain soaked through a mass of earth eight feet thick.

**Shallower vessel.**

Again, he took another vessel, eight inches deep, which he filled with earth and exposed in like manner. No rain penetrated so as to run out at the bottom from June to February;

but after that time it yielded a quantity after most rains. Another vessel of twice the depth, or 16 inches, gave a result much like that of eight inches. Farther, M. de la Hire found, that when herbs were planted in the soil of the last mentioned vessel, and grown up, no rain penetrated through the soil, but instead thereof it was not sufficient to sustain the vegetation; for the plants would require to be sprinkled occasionally, or else they began to droop and wither.

With respect to the first mentioned fact, we need not wonder Remarks. Paris has only 20 inches annual rain; that no water penetrated through eight feet of earth at Paris, where the annual rain is but 20 inches, when only eight or nine inches penetrated through three feet of earth here, where the rain is 33 or 34 inches annually. But it does not follow that rain may not descend down declivities of the ground into but the higher grounds there as well as here may afford waters, &c. vallies or lower parts, at Paris as well as here, and being accumulated may penetrate into the earth to a considerable depth, especially if it meet with channels or chasms of any kind, or declining strata of earth that are impenetrable by water. Paris, I believe, however, is not very liberally supplied with springs, as might be expected. As to the experiment upon vegetation, it only proves that the rain in spring and summer is sometimes not sufficient to support vegetable life, a fact which may readily be granted; but then in his experiment the plants were precluded from a supply of moisture from the earth beneath the vessel, which is a reserve of the utmost consequence in dry seasons.

This circumstance of water ascending again in the earth, Water after soaking into the earth ascends again. on whatever principle it is effected, cannot be denied.—There were  $4\frac{3}{4}$  inches of rain here in July last, none of which passed through the earth in the evaporating vessel; this earth, however, at the end of the month, was far from that degree of dryness which is unfit for the support of vegetation.—During the first four days of August there fell about three inches of rain, and only  $\frac{1}{2}$  an inch penetrated through the earth in the evaporating vessel. Consequently three feet in depth of earth that was moderately moist imbibed nearly three inches of rain before it was saturated; whence we may conclude that three inches nearly had ascended and been evaporated. This evidently shews, that earth is capable of holding a very great proportion of water, that in summer the water ascends to sup-  
ply



ply the exigencies at the surface, and that earth far under the point of saturation with moisture is still fit to support vegetation.

**Question.** How much water is contained in saturated ground. This observation suggested the following question—How much water is there in a given depth of earth when the soil is at the point of saturation, or in that state when it begins to yield water from the lower pipe of the evaporating gage?

**Experiment.** 1 foot saturated earth contains 7 inches water, and it may lose half before it is too dry for vegetation. To determine this I took a quantity of garden soil that had been soaked with rain a day before, and pressed it into a crucible; in this state I found its specific gravity to that of water as five to three. It was then exposed to a moderate heat till it appeared, as near as I could judge, of the same moisture as garden soil two inches deep in dry summer weather; afterwards it was exposed almost to a red heat till it became a perfectly dry powder; in the former case it lost  $\frac{1}{2}$  of its weight, and in the latter  $\frac{1}{3}$ .—When it had lost  $\frac{1}{2}$ , it did not appear too dry to support vegetation. When it had lost  $\frac{2}{3}$ , it appeared like the top soil in summer.—Hence it follows, that every foot of earth in depth, so saturated, contains seven inches of water, and that it may part with one quarter of its water, or even one half, and not be too dry for supporting vegetation.

**Brick clay** nearly the same. Clay, just dug out for the purpose of making bricks, was tried in the same manner: It gave the same specific gravity as the earth, and yielded not much less water.

**De la Hire's** conclusions erroneous. These experiments and observations prove, that M. de la Hire's conclusions, drawn from the vegetation of plants in a given quantity of soil, precluded from any communication with the earth at large, are erroneous, or at least unwarranted: As it does not thence appear that the evaporation for the *whole* year exceeds the rain in the year, whatever it may do for a month or two in summer.

**Springs** are therefore supplied by the rains. The origin of springs may still therefore be attributed to rain, till some more decisive experiments appear to the contrary; and it becomes unnecessary to controvert the other two opinions respecting this subject.

**Springs** are thence deficient in summer. Upon the whole it should seem, that at the commencement of spring, the ground is nearly saturated with water for five or six feet in depth, as the rains and dews in autumn and winter far exceed the evaporation: There are then five or six inches of water at least to be raised up again to the surface in case



case of exigence in the spring and summer : If this happen to be so, then it is at the expence of springs ; for we find the generality of springs become languid, or entirely cease to flow at the end of a long drought. As to the few springs that seem to be little affected by dry or wet seasons, they form exceptions which it would not be difficult to account for.

## VII.

*Description of an Instrument for extracting Hard Substances which may stick during their Passage to the Stomach. By G. C.*

To Mr. NICHOLSON.

SIR,

ENCLOSED, I send you a drawing of an untried instrument ; if you think it likely to accomplish the end for which it was designed, you will, perhaps, give it a place in your very useful work. This instrument, I conceive to be an improvement of that commonly used for forcing down any hard substances that may stick between the mouth and the stomach. In many cases nails, pins, and other metallic matters, get into this situation, when it would undoubtedly be preferable to draw them up through the mouth, instead of passing them into the stomach, where they are no sooner arrived, than they furnish a new species of danger to the sufferer. With this view the following instrument was constructed. A, B, Pl. XI. is a rod of whalebone, having a small groove down the middle, from end to end, large enough to contain a strong silken thread ; this thread is confined to the groove by a few lapings of fine waxed silk. At B, is fastened, as usual, a sponge, about one third of the common size. Just above the sponge is fixed a small pulley, round which the silken thread winds, and returning up the opposite side of the whalebone to that on which it descended, is tied fast to the bottom of a small leather cap C. Above this cap, at D, are fastened 12 or 14 small silver wires, made to spring into the form represented in Fig. 2. These wires by means of loops at their ends, support a round bag of net-work of fine silk, perforated in the centre to admit the whalebone rod. These wires, together with the bag, must be capable of being inserted and confined in the cap C,

Instrument for  
extracting bodies  
from the throat.

Description.

so as to remain in the position represented in Fig. 1. in which state it is ready for being passed down the oesophagus; and it seems scarcely necessary to remark, that previous to the instrument being withdrawn, the cap C must be pulled off, by means of the thread at A; when the wires will expand the net, and press its edge close to the gullet on every side, and in returning to the mouth, will probably bring up any substance that was lodged there, within it.

G. C.

Brompton, Jan. 23, 1803.

## VIII.

*On the Flexure of Wax and other Bodies by irregular cooling, with Considerations on the Probability that it may be caused by the Law of Crystallization. In a Letter from R. B.*

To Mr. NICHOLSON.

London, Feb. 16, 1803.

SIR,

Thread of wax  
flowing down a  
candle,

becomes sepa-  
rated by sponta-  
neous flexure.

The wax was  
cooled by differ-  
ent parts in  
succession.

A STREAM of wax has just overflowed the cup of the wax candle by which I have been reading, and has presented me with a fact or two which I think worthy the meditation of philosophers. If you think so, please to give them a place in your repository.

The fluid wax has formed a line or protuberance on the outside of the candle, four inches in length, a little more than one fifteenth of an inch in width, or surface applied to the candle, and one tenth of an inch in elevation or thickness. As it grew cold it has separated from the candle, so that its lower extremity stands a little more than half an inch distant, and it does not touch for the length of two inches and a half from the bottom; all the upper part still continuing adherent. And lastly, the separated portion exhibits a regular curve, convex towards the candle, and more convex the nearer the lower extremity; so that, when carefully taken off, and applied to several circular arcs described upon paper, the difference was very obviously perceived.

Upon these facts I observe from obvious reasons that the wax which flowed in contact with the solid candle, was more speedily

speedily cooled than that which flowed on the outside of that heated wax; and that that portion of the wax which flowed in contact with the candle, and ran to the greatest distance was the most speedily cooled.

And the result farther shews; 1. That the dimensions of wax suddenly cooled, are larger than those of wax cooled more slowly; and 2. The quicker the cooling the greater this difference. For the fluid wax was deposited in a strait line, and its curve figure after cooling shewed that the interior line or convex limit was longer than the exterior or concave limit; and this difference being greatest where the refrigeration was most sudden, namely, towards the lower extremity, was shewn in the greater curvature.

I will not, Sir, do your readers the injustice to suppose any of them will think the dignity of philosophy impaired by a speculation on the guttering of a candle, but will proceed in my disquisition in hopes that others of more leisure and ability may pursue the object farther, if found to deserve it. And as it has been a fashion since the publication of the famous string of queries at the end of Sir Isaac Newton's Optics, for speculators to use that modest term to dignify inductions which they may suppose to be almost proved, I will take the liberty to offer a few on the present occasion.

1. Since the specific gravity of steel suddenly cooled, is less than when annealed or slowly reduced in temperature; since ice and other crystals slowly formed, are generally understood to be denser than the products of hasty refrigeration; and since in our experiment wax obeys the same law, is it not probable that the law may be general in the cooling of all bodies. *Fiant Experimenta.*

2. Can this effect be ascribed to any thing but the arrangement of the parts of bodies; and if so, is it not referable to hasty and slow crystallization; and are we not therefore justifiable in supposing that the crystallization of bodies may be altered, even in the solid state, as in the hardening or softening of steel.

3. As the hardness of steel becomes greater and its tenacity less by sudden refrigeration; is it not probable that all the products of hasty crystallization are harder as they are known to be more brittle. Try in sal ammoniac whether its softness or flexibility will become less by sudden cooling, such as subli-

Deduction. Wax quickly cooled is less dense than if slowly cooled.

Apologetical remark.

Qu. 1. Are not all bodies rarer if more speedily cooled, congealed or crystallized?

Qu. 2. Is not this crystallization? even in bodies already solid?

Qu. 3. Are not all these bodies more hard and less tenacious?



mation in an head covered with ice; and examine whether an unannealed glass tube be not harder than a portion of the same tube slowly cooled.

Qu. 4. Will it not be useful to repeat this experiment with other bodies?

4. May not some useful indications as to the properties of metals and other bodies be derived from the simple process of casting a long slip of them, by pouring the fused metal upon a cold stone. If the curvature be universally like that of the wax, the law will be more and more confirmed. If anomalies present themselves, our acquaintance with natural events will nevertheless be extended and improved.

Experiments.

Since writing the above, I have taken some of the materials next at hand to make an experiment or two, which I give without regarding whether they support, modify, or destroy the hypothesis advanced above.

Exp. 1. Annealed and unannealed glass, The annealed seems hardest.

Exp. 1. A thick glass tube which had been bended into a syphon, was broken in two at the place of flexure. It was supposed that the bended part having been heated and cooled a second time, might prove softer from this kind of annealing. The extremity of the strait part was applied to scratch the bended part, and also the other strait part; and contrarywise the extremity of one of the bended parts was applied to the other portion of tube; and lastly both ends were scratched with a file. No certainty was obtained, but it was thought that the bended part was hardest\*.

Exp. 2. Lead cast on a slab. No result.

Exp. 2. Lead was fused and poured red hot upon a marble slab in a long slip. Other lead moderately heated was poured out. The pieces did not quit the face of the stone in cooling, and they were too flexible to be taken up and examined.

Exp. 3. Type metal. Bended like the wax.

Exp. 3. Type metal was treated in the same manner. A flat piece one thirtieth of an inch thick, and six inches long, bended upwards from the stone in cooling, to the height or versed sine of one twentieth of an inch, and retained its flexure.

Exp. 4. Fusible metal. Bended very much when nearly cold, contrary to the wax, and recovered itself afterwards.

Exp. 4. Fusible metal or the compound of lead, tin, and bismuth, was poured out in a slip 13 inches long, one half being about the same thickness or less than the type metal,

\* As the tube had been bended with the blow pipe, the difference might as well have arisen from oxygenation of the lead or manganese, or dissipation of alkali, as from simple heating and cooling.

R. B.

and

and the other half considerably thicker. It did not appear to rise from the slab; but was taken up while quite warm and held with its edge downwards, to prevent any flexure from its weight: The thin part immediately began to bend, and the thick part soon afterwards, *the flexures being concave on the side which had touched the stone*, and the versed sine or height of the curvature was one inch at the end of half a minute, after which it gradually straightened itself as follows,

*Bar of Fusible Metal 13 Inches long.*

At 8 <sup>h</sup> 51 <sup>m</sup>	taken up strait.	inch.
51 $\frac{1}{2}$	it became bended; versed sine	1.00
54	- - less bended - do. -	0.7
55	- - _____	0.4
57	- - _____	0.25
60	thin part strait; thick a little curved.	

Another experiment had been made before, without any expectation of such or so great a flexure, and no measures were then taken. In this the bar was thinner and the flexure greater; and the return to straightness was not only complete, but one end which was very thin became at last bended the contrary way, namely the face that had touched the stone was convex.

A smaller piece of two inches in length which was not taken up so soon, had become bended as it lay; so that it rested on its two extremities and was hollow in the middle. This recovered its first straightness as it lay on the table.

I am

S I R,

your obliged reader,

R. B.

## IX.

*An Account of some Cases of the Production of Colours, not hitherto described. By THOMAS YOUNG, M. D. F. R. S. F. L. S. Professor of Natural Philosophy, in the Royal Institution \*.*

Simple and general law respecting two portions of the same light arriving by different routes ;

WHATEVER opinion may be entertained of the theory of light and colours which I have lately had the honour of submitting to the Royal Society, it must at any rate be allowed that it has given birth to the discovery of a simple and general law, capable of explaining a number of the phenomena of coloured light, which, without this law, would remain insulated and unintelligible. The law is, that “wherever two portions of the same light arrive at the eye by different routes, either exactly or very nearly in the same direction, the light becomes most intense when the difference of the routes is any multiple of a certain length, and least intense in the intermediate state of the interfering portions ; and this length is different for light of different colours.”

which explains many phenomena.

and new facts.

I have already shown in detail, the sufficiency of this law for explaining all the phenomena described in the second and third books of Newton's Optics, as well as some others not mentioned by Newton. But it is still more satisfactory to observe its conformity to other facts, which constitute new and distinct classes of phenomena, and which could scarcely have agreed so well with any anterior law, if that law had been erroneous or imaginary : these are, the colours of fibres, and the colours of mixed plates.

Production of colours by a minute fibre near the edge of an obstacle intercepting light &c.

As I was observing the appearance of the five parallel lines of light which are seen upon the margin of an object held near the eye, so as to intercept the greater part of the light of a distant luminous object, and which are produced by the fringes caused by the inflection of light already known, I observed that they were sometimes accompanied by coloured fringes, much broader and more distinct ; and I soon found, that these broader fringes were occasioned by the accidental interposition of a hair. In order to make them more distinct, I employed a horse-hair ; but they were then no longer visible. With a

\* Philos. Trans. 1802.



fibre of wool, on the contrary, they became very large and conspicuous: and, with a single silk-worm's thread, their magnitude was so much increased, that two or three of them seemed to occupy the whole field of view. They appeared to extend on each side of the candle, in the same order as the colours of thin plates, seen by transmitted light. It occurred to me, that their cause must be sought in the interference of two portions of light, one reflected upon the fibre, the other bending round its opposite side, and at last coinciding nearly in direction with the former portion; that accordingly as both portions deviated more from a rectilinear direction, the difference of the length of their paths would become gradually greater and greater, and would consequently produce the appearances of colour usual in such cases; that, supposing them to be inflected at right angles, the difference would amount nearly to the diameter of the fibre, and that this difference must consequently be smaller as the fibre became smaller; and, the number of fringes in a right angle becoming smaller, that their angular distances would consequently become greater, and the whole appearance would be dilated. It was easy to calculate, that for the light least inflected the difference of the paths would be to the diameter of the fibre, very nearly as the deviation of the ray, at any point, from the rectilinear direction, to its distance from the fibre.

Remarks and inferences. A portion of light reflected from one side nearly coincides in direction with another portion inflected; but their lengths differ and produce colour.

I therefore made a rectangular hole in a card, and bent its ends so as to support a hair parallel to the sides of the hole; then, upon applying the eye near the hole, the hair of course appeared dilated by indistinct vision into a surface, of which the breadth was determined by the distance of the hair and the magnitude of the hole, independently of the temporary aperture of the pupil. When the hair approached so near to the direction of the margin of a candle that the inflected light was sufficiently copious to produce a sensible effect, the fringes began to appear; and it was easy to estimate the proportion of their breadth to the apparent breadth of the hair, across the image of which they extended. I found that six of the brightest red fringes, nearly at equal distances, occupied the whole of that image. The breadth of the aperture was  $\frac{66}{1000}$ , and its distance from the hair  $\frac{8}{10}$  of an inch: the diameter of the hair was less than  $\frac{1}{500}$  of an inch; as nearly as I could ascertain, it was  $\frac{1}{600}$ . Hence we have  $\frac{1}{1000}$  for the

Precise repetition of the experiment

the deviation of the first red fringe at the distance  $\frac{8}{10}$ ; and, as  $\frac{8}{10} : \frac{11}{1000} :: \frac{1}{600} : \frac{11}{480000}$ , or  $\frac{1}{43636}$  for the difference of the routes of the red light where it was most intense. The measure deduced from Newton's experiments is  $\frac{1}{39200}$ . I thought this coincidence, with only an error of one-ninth of so minute a quantity, sufficiently perfect to warrant completely the explanation of the phenomenon, and even to render a repetition of the experiment unnecessary; for there are several circumstances which make it difficult to calculate much more precisely what ought to be the result of the measurement.

The halos  
round a distant  
candle seen  
through wool.

When a number of fibres of the same kind, for instance, a uniform lock of wool, are held near to the eye, we see an appearance of halos surrounding a distant candle: but their brilliancy, and even their existence, depends on the uniformity of the dimensions of the fibres; and they are larger as the fibres are smaller. It is obvious that they are the immediate consequences of the coincidence of a number of fringes of the same size, which, as the fibres are arranged in all imaginable directions must necessarily surround the luminous object at equal distances on all sides, and constitute circular fringes.

Coloured atmos-  
pheric halos.

There can be little doubt that the coloured atmospheric halos are of the same kind: their appearance must depend on the existence of a number of particles of water, of equal dimensions, and in a proper position, with respect to the luminary and to the eye. As there is no natural limit to the magnitude of the spherules of water, we may expect these halos to vary without limit in their diameters; and, accordingly, Mr. Jordan has observed that their dimensions are exceedingly various, and has remarked that they frequently change during the time of observation.

New colours  
seen through two  
plates of glass  
with a little  
moisture between  
them mixed  
with air.

I first noticed the colours of mixed plates, in looking at a candle through two pieces of plate-glass, with a little moisture between them. I observed an appearance of fringes resembling the common colours of thin plates; and, upon looking for the fringes by reflection, I found that these new fringes were always in the same direction as the other fringes, but many times larger. By examining the glasses with a magnifier, I perceived that wherever these fringes were visible, the moisture was intermixed with portions of air, producing an appearance similar to dew. I then supposed that the origin of the colours was the same as that of the colours of halos; but, on

a more minute examination, I found that the magnitude of the portions of air and water was by no means uniform, and that the explanation was therefore inadmissible. It was, however, easy to find two portions of light sufficient for the production of these fringes; for, the light transmitted through the water, moving in it with a velocity different from that of the light passing through the interstices filled only with air, the two portions would interfere with each other, and produce effects of colour according to the general law. The ratio of the velocities in water and in air, is that of 3 to 4; the fringes ought therefore to appear where the thickness is 6 times as great as that which corresponds to the same colour in the common case of thin plates; and, upon making the experiment with a plane glass and a lens slightly convex, I found the sixth dark circle actually of the same diameter as the first in the new fringes. The colours are also very easily produced, when butter or tallow is substituted for water; and the rings then become smaller, on account of the greater refractive density of the oils: but, when water is added, so as to fill up the interstices of the oil, the rings are very much enlarged; for here the difference only of the velocities in water and in oil is to be considered, and this is much smaller than the difference between air and water. All these circumstances are sufficient to satisfy us with respect to the truth of the explanation; and it is still more confirmed by the effect of inclining the plates to the direction of the light; for then, instead of dilating, like the colours of thin plates, these rings contract: and this is the obvious consequence of an increase of the length of the paths of the light, which now traverses both mediums obliquely; and the effect is every where the same as that of a thicker plate.

These were not produced by reflection and refraction but by the different velocities of the light through water and air.

Proofs of this theory.

It must however be observed, that the colours are not produced in the whole light that is transmitted through the mediums: a small portion only of each pencil, passing through the water contiguous to the edges of the particle, is sufficiently coincident with the light transmitted by the neighbouring portions of air, to produce the necessary interference; and it is easy to show that, on account of the natural concavity of the surface of each portion of the fluid adhering to the two pieces of glass, a considerable portion of the light which is beginning to pass through the water will be dissipated laterally by reflection.

The curved figure of the particles cause much of the light to deviate and modify the effects.



tion at its entrance, and that much of the light passing through the air will be scattered by refraction at the second surface. For these reasons, the fringes are seen when the plates are not directly interposed between the eye and the luminous object; and, on account of the absence of foreign light, even more distinctly than when they are in the same right line with that object. And, if we remove the plates to a considerable distance out of this line, the rings are still visible, and become larger than before; for here the actual route of the light passing through the air, is longer than that of the light passing more obliquely through the water, and the difference in the times of passage is lessened. It is however impossible to be quite confident with respect to the causes of these minute variations, without some means of ascertaining accurately the forms of the dissipating surfaces.

The arguments from the general law shew that the velocity of undulation is greatest in rare mediums,

The central black spot on a soap bubble is produced by undulations reflected from the confines of a dense and a rare medium in circumstances to destroy each other :

a contrary effect is produced when both reflections are from a rarer medium.

In applying the general law of interference to these colours, as well as to those of thin plates already known, I must confess that it is impossible to avoid another supposition, which is a part of the undulatory theory, that is, that the velocity of light is the greater, the rarer the medium; and that there is also a condition annexed to the explanation of the colours of thin plates, which involves another part of the same theory, that is, that where one of the portions of light has been reflected at the surface of a rarer medium, it must be supposed to be retarded one half of the appropriate interval, for instance, in the central black spot of a soap-bubble, where the actual lengths of the paths very nearly coincide, but the effect is the same as if one of the portions had been so retarded as to destroy the other. From considering the nature of this circumstance, I ventured to predict, that if the two reflections were of the same kind, made at the surfaces of a thin plate, of a density intermediate between the densities of the mediums containing it, the effect would be reversed, and the central spot, instead of black, would become white; and I have now the pleasure of stating, that I have fully verified this prediction, by interposing a drop of oil of saffrafs between a prism of flint-glass and a lens of crown glass: the central spot seen by reflected light was white, and surrounded by a dark ring. It was however necessary to use some force, in order to produce a contact sufficiently intimate; and the white spot differed, even at last, in the same degree from perfect whiteness, as the black spot usually does from perfect blackness.

The colours of mixed plates suggest to me an idea which appears to lead to an explanation of the dispersion of colours by refraction, more simple and satisfactory than that which I advanced in the last Bakerian lecture. We may suppose that every refractive medium transmits the undulations constituting light in two separate portions, one passing through its ultimate particles, and the other through its pores; and that these portions re-unite continually, after each successive separation, the one having preceded the other by a very minute but constant interval, depending on the regular arrangement of the particles of a homogenous medium. Now, if these two portions were always equal, each point of the undulations resulting from their re-union, would always be found half way between the places of the corresponding point in the separate portions; but, supposing the preceding portion to be the smaller, the newly combined undulation will be less advanced than if both had been equal, and the difference of its place will depend, not only on the difference of the length of the two routes, which will be constant for all the undulations, but also on the law and magnitude of those undulations; so that the larger undulations will be somewhat further advanced after each re-union than the smaller ones, and, the same operation recurring at every particle of the medium, the whole progress of the larger undulations will be more rapid than that of the smaller; hence the deviation, in consequence of the retardation of the motion of light in a denser medium, will of course be greater for the smaller than for the larger undulations. Assuming the law of the harmonic curve for the motions of the particles, we might without much difficulty reduce this conjecture to a comparison with experiment; but it would be necessary, in order to warrant our conclusions, to be provided with very accurate measures of the refractive and dispersive powers of various substances, for rays of all descriptions.

Dr. Wollaston's very interesting observations would furnish great assistance in this inquiry, when compared with the separation of colours by thin plates. I have repeated his experiments on the spectrum with perfect success, and have made some attempts to procure comparative measures from thin plates; and I have found that, as Sir Isaac Newton has already observed, the blue and violet light is more dispersed by refraction, than in proportion to the difference of the appropriate dimensions

Dispersion of colours by refraction explained from the assumptions that light passes through the particles and through the pores.

Comparison of Dr. Wollaston's obs. with the separation of colours by thin plates.

dimensions deduced from the phenomena of thin plates. Hence it happens, that when a line of the light proceeding to form an image of the rings of colours of thin plates, is intercepted by a prism, and an actual picture is formed, resembling the scale delineated by Newton from theory, for estimating the colours of particles of given dimensions, the oblique spectrums, formed by the different colours of each series, are not straight, but curved, the lateral refraction of the prism separating the violet end more widely than the red. The thickness corresponding to the extreme red, the line of yellow, bright green, bright blue, and extreme violet, I found to be inversely as the numbers 27, 30, 35, 40, and 45, respectively. In consequence of Dr. Wollaston's correction of the description of the prismatic spectrum, compared with these observations, it becomes necessary to modify the supposition that I advanced in the last Bakerian lecture, respecting the proportions of the sympathetic fibres of the retina; substituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6.

Subdivision of  
the light of a  
candle explain-  
ed.

The same prismatic analysis of the colours of thin plates, appears to furnish a satisfactory explanation of the subdivision of the light of the lower part of a candle: for, in fact, the light transmitted through every part of a thin plate, is divided in a similar manner into distinct portions, increasing in number with the thickness of the plate, until they become too minute to be visible. At the thickness corresponding to the ninth or tenth portion of red light, the number of portions of different colours is five; and their proportions, as exhibited by refraction, are nearly the same as in the light of a candle, the violet being the broadest. We have only to suppose each particle of tallow to be, at its first evaporation, of such dimensions as to produce the same effect as the thin plate of air at this point, where it is about  $\frac{1}{100000}$  of an inch in thickness, and to reflect, or perhaps rather to transmit, the mixed light produced by the incipient combustion around it, and we shall have a light completely resembling that which Dr. Wollaston has observed. There appears to be also a fine line of strong yellow light, separate from the general spectrum, principally derived from the most superficial combustion at the margin of the flame, and increasing in quantity as the flame ascends. Similar circumstances might undoubtedly be found in other cases of the production or modification of light; and experiments upon this subject might



might tend greatly to establish the Newtonian opinion, that the colours of all natural bodies are similar in their origin to those of thin plates; an opinion which appears to do the highest honour to the sagacity of its author, and indeed to form a very considerable step in our advances towards an acquaintance with the intimate constitution and arrangement of material substances.

I have lately had an opportunity of confirming my former observations on the *Dispersive powers of the eye.* I find that at the respective distances of 10 and 15 inches, the extreme red and extreme violet rays are similarly refracted, the difference being expressed by a focal length of 30 inches. Now the interval between red and yellow is about one-fourth of the whole spectrum; consequently, a focal length of 120 inches expresses a power equivalent to the dispersion of the red and yellow, and this differs but little from 132, which was the result of the observation already described. I do not know that these experiments are more accurate than the former one; but I have repeated them several times under different circumstances, and I have no doubt but the dispersion of coloured light in the human eye is nearly such as I have stated it. How it happens to be no greater, I cannot at present undertake to explain.

## X.

*A Memoir on the Wax Tree of Louisiana and Pennsylvania.* By CHARLES LOUIS CADET, of the College of Pharmacy at Paris\*.

A GREAT number of plants, such as the *croton sebiferum*, *Wax bearing plants.* the *tomex sebifera* of Loureiro, the poplar, the alder, the pine, and several labiated plants, afford a concrete inflammable matter by decoction, more or less resembling tallow or wax, that is to say, a fixed oil saturated with oxygen. The light matter, which is called the down of fruits, which silvers the surface of prunes and other stone fruits, is wax, as Mr. Proust has shewn. But the tree which presents this substance

\* Translated from the Ann. de Chimie, XLIV. 140.

The most productive is the *myrica cerifera*.

in the greatest abundance, and in more respects than one, is entitled to the attention of cultivators, is the *myrica cerifera*, or wax tree.

Its early history.

We read in the History of the Academy of Sciences for the years 1722 and 1725, that M. Alexandre, surgeon, correspondent with M. Marian, had observed at Louisiana a tree of the size of a cherry tree, having the appearance of the myrtle, and bearing a grain of the size of coriander seed. These grains, of a grey ash colour, contain a small round hard kernel, which is covered with a shining wax that may be obtained by boiling the grains in water. This wax is drier and more friable than ours. The inhabitants make candles of it. M. Alexandre adds, "This grain is usually of a deep and beautiful lake colour, which by merely crushing with the fingers leaves them tinged, but this is only at a particular season."

The liquor in which the grain has been boiled, and from whence the wax is procured, having been poured out and evaporated to the consistence of an extract, M. Alexandre discovered that it checks the most obstinate dysenteries.

The advantageous properties that this tree appears to possess, ought to have induced philosophers to make enquiries to ascertain the various properties of the vegetable, and what attention its culture might require; it has been long considered merely as an object of curiosity.

Linnaeus, in his vegetable system, only mentions the wax tree of Virginia, *myrica cerifera*, the leaves lanceolated as if dentated, and the stem arborescent.

Species of the *myrica cerifera*.

On enquiring of Cit. Ventenat, whether there were several species, he informed me, that Ayton distinguishes two, viz.

1. The *myrica cerifera angustifolia*, which grows in Louisiana. This tree is very delicate, it flowers with difficulty in our green houses; and its grains are smaller than those of the following.

2. The *myrica cerifera latifolia*, which grows in Pennsylvania, Carolina, and Virginia, does not rise so high as the former; it is perfectly naturalized in France. These two *myrica* are diæcous.

They are both cultivated in the *Muséum des Plantes*, and in the gardens of Citizens Cels and Lemonier (at Paris.)

Cit.

Cit. Michault admits of a third species of *myrica cerifera*, which he calls the dwarf wax tree. Cit. Ventenat believes that wax may be procured from all the myrica.

The authors who have mentioned these trees with some detail are Marshal, translated (into French) by Lesferme, Lepage-Duprat, and Toscan, librarian at the Museum of Natural History. A memoir inserted by the last in his work intitled *L'Ami de la Nature*, shews the manner in which the vegetable wax is collected in the colonies.

Authors who have written respecting it.

"Towards the end of autumn," says he, "when the berries are ripe, a man leaves his house, together with his family, to go to some island or bank on the sea shore where the wax trees grows in abundance. He carries with him vessels to boil the berries, and a hatchet to build a cottage where he may find shelter during his residence in this place, which is usually three or four weeks. While he cuts down the trees his children gather the berries. A very fertile shrub will afford near seven pounds. When these are gathered the whole family employ themselves in procuring the wax. They throw a certain quantity of berries into the kettle, and then pour a sufficient quantity of water on them so as to cover them to a depth of about half a foot. They boil the whole, stirring the grains about and rubbing them against the sides of the vessel, in order that the wax may more easily come off. In a short time it floats on the water like fat, and is collected with a spoon and strained through a coarse cloth to separate it from any impurities which might be mixed with it. When no more wax can be obtained, they take the berries out with a skimmer, and put others into the same water; but it must be entirely changed the second or third time, and in the mean time boiling water must be added as it evaporates, in order to avoid retarding the operation. When a considerable quantity of wax has been obtained by this means, it is laid upon a cloth to drain off the water with which it is still mixed. It is then dried and melted a second time, to render it more pure, and it is then formed into masses. Four pounds of berries afford about one of wax; that which is first obtained is generally yellow: but in the latter boilings it assumes a green colour from the pellicle with which the kernel of the berry is covered."

Manner of collecting the wax in America.



It is there used for soap making. The traveller Kalm, speaking of the vegetable wax, says, that in the country where the wax tree grows they make excellent soap of it, which washes linen perfectly white.

The author obtains some of the wax; Such are the notions which have been formed respecting the myrica; at least no other observations had been published to my knowledge, when a naturalist favoured me with half a kilogramme ( $17\frac{1}{2}$  oz. avoird.) of the vegetable wax of Louisiana. I was desirous of making a comparative analysis of it with bees wax; but before I undertook this work, I wished to see the tree and berry of the myrica. I saw this precious vegetable at the *Jardin des Plantes*, and I wrote to Cit. Deshayes, a zealous botanist, who cultivates the myrica pennsylvanica at Rambouillet, to request him to give me some information concerning it. He had the politeness to send me an answer with some of the berries, which I immediately examined.

and examines the grain. This grain is a kind of berry of the size of a pepper corn; the outside when it is ripe and fresh, is white, and covered with small black asperities which give it the appearance of shagreen. When it is rubbed in the hands, it renders them greasy or unctuous.

Experiments to separate the waxy part mechanically. If one of these little berries be strongly pressed, it parts with a matter resembling starch, and mixed with small brown round grains like fine gunpowder. The nut which remains bare has a very thick ligneous covering, and contains a dycolitidinous kernel. By rubbing a handful of the berries on a sieve of horse hair, I obtained a grey powder, in which the eye distinguishes without the assistance of a magnifying glass, the small brown grains I mentioned, mixed with a white powder.

Application of alcohol. I put this powder into alcohol, which with the assistance of a slight heat, dissolved all the white part, and left the black powder which I separated. Water poured on this solution in alcohol formed a precipitate that floated on the surface of the liquid. I melted this and obtained a yellow wax resembling that sent me from Louisiana. This experiment completely proves that the wax of the myrica is the white grumous matter that surrounds the berries.

Black powder from the surface of the berries. The black powder which was separated appeared to me to contain the colouring principle, and I hoped also to find in it

it the beautiful lake mentioned by M. Alexandre. With this notion I crushed the powder strongly, and boiled it in a solution of acid sulphate of alumine; but was very much surprised to obtain nothing but a liquor scarcely coloured, and by which the alumine, when precipitated by an alkali, was but slightly tinged.

I took another portion of this fine black powder, and infused it in alcohol, I soon obtained a tincture of the colour of wine lees, which on being heated, became as red as a strong tincture of quinquina, or terra japonica. The result led me to think that the colouring principle was resinous; but on adding water to it, I did not perceive that any precipitate was formed.

I then poured into this tincture water charged with sulphate of alumina, and obtained slight precipitate. A solution of sulphate of iron immediately formed ink.

What may this astringent colouring principle be which is only soluble in alcohol, which is not precipitated by water, and has so little attraction for alumine? To discover it, it is necessary to make a course of experiments which the small quantity of the substance I was in possession of would not permit me to undertake. The astringent matter mentioned by M. Alexandre must be found in the decoction of the entire grain. To ascertain this fact I boiled some in a silver saucepan, the decoction upon which floated a little of the wax, was of a greenish colour, its taste slightly styptic, and it precipitated black ferruginous solutions. I heated it in an iron vessel for that purpose, and it immediately became black. To discover whether this property arose from the gallic acid alone, or from tannin, I mixed a little of the concentrated decoction with a solution of gelatine, but it afforded no precipitate.

The powder afforded an astringent solution by alcohol.

It contained gallic acid but no tannin,

It is therefore to the considerable quantity of gallic acid which the berries of the myrica contain, that we must attribute its effect in dysenteries. I therefore suppose that the bark and leaves of the tree must contain an extract much more astringent than the berries.

The examination of the wax afforded extremely interesting results.

Examination of the wax.

In whatever manner the wax may be obtained; by the decoction of the grains, or the solution of the powder when precipitated from alcohol by water, this wax when melted is al-

It is greenish yellow; firm, and more brittle than bees wax, burns well in candles, and emits a fragrant smell.

In distillation it is affected like bees wax.

Ether dissolves it better than alcohol.

Ox. mur. acid bleaches it.

It is soluble in ammonia;

and forms soap with fixed alkali.

Its whiteness in soap.

Chemical observation on the oxygenation of the oil in soap.

ways of a greenish yellow. It is of a firmer consistence than bees wax, it is dry, and sufficiently friable to be pulverized. In a word, it is evidently more oxygenated than the wax prepared by bees. Candles made of the wax of the myrica afford a white flame, a good light, without smoke, and do not gutter; they emit when quite fresh a balsamic odour, which the inhabitants of Louisiana consider as extremely wholesome for persons in ill health. When distilled in a retort this wax passes over for the most part in the form of butter. This portion is much whiter, and has no more consistence than tallow. Another portion that was decomposed afforded a little water, with some empyreumatic oil, and sebacic acid. Much carbonated hydrogen gas, and carbonic acid gas were disengaged; there remained in the retort a black and coaly bitumen. Wax usually is affected in the same manner by distillation.

I have already mentioned, that alcohol dissolves the wax of the myrica, but ether dissolves it better, and it separates in the form of stalagmites by the evaporation of the liquid; neither of them discolour it. If this wax be boiled with weak sulphuric acid it becomes paler, but there is no evident combination of the acid with it. The yellow bees wax under the same circumstances did not change colour.

Oxygenated muriatic acid renders both kinds of wax perfectly white. The vegetable wax is the most difficult to be bleached.

Vegetable wax dissolves in ammonia; the solution is of a brown colour; a portion of the wax is saponified. Volatile alkali has much less action on bees wax.

Both kinds of wax when strongly agitated in a boiling solution of caustic potash, become white and form a real soap as Kalm observed.

The whiteness acquired by the wax during this saponification is not a new phenomenon. Chaptal in his process for bleaching by the vapor of alkaline leys, proves that the colouring principle of vegetables yields to the action of alkalis. Some chemists have attributed this effect to the direct combination of the soda or potash with the coloured extract, a combination which makes it almost of a soapy quality, and renders it soluble.

I apprehend that in this operation, the alkali exercises a double attraction on the oil or wax, first directly with the constituent



stituent principles of the oil and then predisposing, and favouring the combination of the atmospheric oxygen with the oil or wax. I do not know whether my notion is original; but I deduced it from the observation of what passes during the decomposition of a soap by an acid: the oil is always concrete and becomes oxygenated. It would be interesting for chemical theory to make, if it were possible, a soap in a closed apparatus the air of which might be examined after the experiment, or in different gases not containing oxygen.

When the soap of myrica is decomposed a very white wax is obtained, but in a peculiar state rendering it unfit for our uses. The veg. wax from its soap is in a peculiar state.

Litharge or the semi-vitreous oxide of lead dissolves very well in melted wax of Louisiana; it forms a very hard plaister, but its consistence may be diminished at pleasure by the addition of a little oil. If as there is reason to suppose, the wax of myrica retains a portion of the astringent principle afforded by the decoction of the berries, physicians may perhaps discover some useful topical remedies in the compounds of this wax. Plasters with veg. wax.

From what has been said, we see that the myrica may be of the greatest use to the arts. The wax which it affords is sufficiently abundant to recompense the care and expense of cultivating it. For a bush in a full bearing yields from six to seven pounds of kernels, one fourth of which may be obtained in wax. It is superior in quality to bees-wax. Great utility of this wax.

The astringent principle of the myrica, extracted in the large way may be very useful in medicine and in the arts; it may to a certain extent be substituted instead of nut galls in dyeing, hat making, and probably in certain processes of tanning. The colouring principle appears sufficiently solid to deserve some attention; and if it be true that some fine lakes have been obtained from it in Louisiana, why may we not expect advantages from it in painting. and of its astringent matter.

Lastly when this wax shall have become plentiful and cheap in the market it promises great advantage in the fabrication of soap.

The art of bleaching this wax will also require a course of experimental research; if it be proposed to operate economically and in the large way. Two re-agents present themselves to manufacturers; the sulphuric acid, and the oxygenated To bleach veg. wax.

muriatic acid. But as the wax does not sink in these liquids, we must multiply the contacts either by slicing and sprinkling it with the oxygenated muriatic acid, or by inclosing it in this divided state in casks into which the oxygenated muriatic gas should be passed.

The preference given to the action of the ox. mur. of lime.

I shall propose a third which promises a speedier effect. The wax in a very divided state is to be stratified in a cask, with super-oxygenated muriate of lime; in this manner they are to be disposed in strata, and left for some time in contact without moisture. The salt is afterwards to be decomposed with water acidulated by the sulphuric acid; taking care to pour the water a little at a time at different intervals, until there shall be no longer any perceptible disengagement of muriatic gas; at which period a large quantity of water is to be added and the mixture agitated with a staff. The insoluble sulphate of lime falls down by repose, while the bleached wax rises and swims at the surface. This is to be washed and melted on the water bath.

On the culture of the tree in Europe.

I shall conclude this memoir by offering some notions respecting the culture of the *myrica pensylvanica*.

Cit. Deshayes to whom I am indebted for the trials I have made has observed the wax tree for several years at Ram-bouillet. The following is what he writes to me on the subject.

It is perfectly at home in France.

“The *myrica (latifolia)* Ayton is here absolutely in its native country; it is in the soil best suited to it, namely in a sandy and blackish turf: we have sixteen very flourishing wax trees. Their height is from four to five and six feet, and one of them a male is seven feet high. The berries are abundant almost every year, I say almost because there are some years in which they have failed. In general this fruit thrives very well in the part of the English garden assigned to it.

and requires no care in its culture.

“The culture requires no care. Every year numerous shoots are taken off which rise at the feet of the large wax trees. These are planted in other parts of the garden at the distance of one metre or yard asunder.”

The grain may be sowed in the ground in spring, and afterwards transplanted, but the process would be too long. The *myrica* succeeds wherever the soil is light and rather moist. How many provinces are there in which this cultivation might become useful and give employment to lands at present nearly abandoned.



How great are the advantages our agriculture might expect from this acquisition, since the myrica has long ago been seen to flourish in the dry sands of Prussia. Cit. Thieboult of the academy of Berlin, has communicated this interesting fact to me in the following note.

The late Mr. Sulzer author of the general dictionary of the fine arts, obtained from Frederic the Great a portion of waste ground of considerable extent on the banks of the Spree, a league from Berlin, in a place called the Moabites. However ungrateful this soil might appear as it presented only a very scanty and thin turf, upon a fine and light sand, Mr. Sulzer converted it into a garden very agreeable and worthy of a philosopher. Among other remarkable things, he made a plantation of foreign trees composed of five long rows, in the direction from East to West. There were not two trees in succession of the same species; he had placed in the rows most exposed to the North, none but such as were loftiest and most capable of resisting the rigours of the climate, So that by proceeding from North to South, the first row presented only trees of about seventy feet in height, the second trees between twenty-five and thirty feet high, and so in succession in an amphitheatre, where all the trees enjoyed the sun, at least in a part, and the weakest were sheltered by those which were more hardy.

*Narrative of a plant in the garden of Mr. Sulzer.*

It was in the southernmost row, that I observed a kind of bush only two or three feet high, which Mr. Sulzer called the wax tree. All the visitors took particular notice of this tree in preference to all others, on account of the delicious odour of its leaves, which they preserved a very long time.

Citizen Thieboult afterwards speaks of the extraction of the wax. This operation does not differ from that related by Mr. Alexander.

I have seen, adds he afterwards, a single candle of this wax perfume the three chambers which composed the particular apartment of Mr. Sulzer, not only during the time it was lighted, but also for the rest of the evening.

*Remarkable perfume from the candles of veg. wax.*

Without doubt the myrica cultivated at Berlin, was more odoriferous than that which grows with us, for ours does not emit the same perfume. Mr. Sulzer had the project of making candles of this wax, not bleached, but covered with our wax for the sake of beauty. The heirs of the academician have



fold the garden, but the wax tree still remained. It was planted in 1770. Since the possibility of naturalizing the *myrica cerifera* has been ascertained in the north, why should we neglect a vegetable of such value and importance which could not fail to prosper in our southern departments, and demands much less care than our bee-hives. The successful experiments already made ought to excite the zeal of our cultivators.

Economical  
Remarks.

The government has already encouraged this branch of industry, by ordering plantations to be made. There exists at Orleans and at Rambouillet, two orchards of the wax tree which contain more than four hundred shrubs. We cannot give too much publicity to such satisfactory results. Nothing is propagated with so much slowness as useful plants. A barren but picturesque tree, or an agreeable flower are soon adopted by the fashion. They ornament the gardens of our modern Luculluses, and the toilets of our Phrynes, while our indefatigable agriculturists make vain efforts to enrich our gardens with a new gramineous plant, or to fill our barns with nourishing cereal plants. The people has long rejected from prejudice both maize and the potatoe which have been so highly serviceable to the poor and to our soldiery. We no longer find in our forests the food bearing oak, upon which our ancestors subsisted. Let us hope that our cultivators will at last open their eyes upon their true interests, and that less enslaved by old practices they will not despise the presents which learned societies are desirous of making for their profit, and the reputation and prosperity of their country.

## XI.

*Outline of the Craniognomic System of Dr. Gall\*, Physician at Vienna. By Dr. BOJAMES†.*

THE desire of finding in the external structure of man, Various systems of physiognomy, certain indications of his internal faculties, his passions, his moral disposition, &c. has in the most remote as well as in modern times, engaged philosophers to establish systems of physiognomy, which have been more or less satisfactory.

The most generally known are those of Porta, Lavater, the of Porta, of Lavater, and of the facial angle. theory of the facial angle, and lastly the system of Gall.

With regard to the first, who has busied himself in comparing the outlines of the figure of man, with that of brutes, System of Porta, hasty and inaccurate. observers have decided on its value, and consider his principles as the product of a wild imagination; they have found them too hasty, very little established on reasonable observations, and absolutely uncertain in application.

Lavater's system has had more success; but though we re- Lavater's system founded on sentiment, not science. vere the genius of this man who was really a great observer, we cannot be ignorant of the loose foundation on which all his opinions are built, and the mind is unsatisfied with truths which can only be appreciated by an imagination as exalted, and feelings as delicate as those of the author.

The theory of the facial angle which comprehends a more Theory of the facial angle true; but too general. ample field than the system of Lavater, leaves us in uncertainty as to the detail of faculties, and gives us only general points of view. But it presents this most important truth, that the facial angle increases in magnitude in equal proportion with the faculties of animals, and in this point it evidently agrees with the general results of Gall's system.

Without entering into a scrupulous detail of the laborious System of Gall its fundamental principles. course which this learned philosopher has followed, in order

\* This historical exposition which does not in the least tend to prove the truths of Gall's system, should not influence any one's judgment concerning it, as it will be confirmed by its author with solid reasoning and convincing proofs.

It is likewise necessary to remark, that the sentences marked with inverted commas do not rest on the authority of Gall.

† Inserted by him in the *Encyclopedie Methodique*, of the learned *Millin*.

to establish certain bases for a science hitherto so hypothetical, I shall content myself with a short examination of its fundamental principles, which are,

1st. *The Brain is the material Organ of the Internal Faculties.*

The brain is the organ of the faculties.

Without endeavouring to decide upon the metaphysical questions respecting the nature of the soul, or of that which may be supposed the occult cause of the internal faculties, we are nevertheless compelled to admit a material organ for their action.

Proofs.

Now when we remark that these faculties are found only where the brain exists, that they are lost with it; that the disorders and injuries sustained by this organ, very sensibly influence their degree and their action; that the volume of the brain increases in direct proportion with the faculties of animals, &c. when we observe all this I say, there is nothing of conjecture in supposing the brain to be their material and intermediate organ.

*Note.* It might here be objected that in many cases individuals have lost a considerable portion of the substance of the brain, without their faculties having been sensibly diminished; but it must be observed, that in general the organs of the brain are double, and that the cases are far from being accurately stated or established.

The brain consists of independent organs.

2d. *The Brain contains different Organs independent\* of each another, for the different Faculties.*

The internal faculties do not always exist in equal proportions with respect to each other; there are men who have much intelligence without much memory, courage without circumspection, and metaphysical genius without being profound observers.

Proofs. The faculties are exerted independently of each other, &c.

Again the phenomena of dreams, of somnambulism, of madness, &c. prove that the internal faculties do not always act together, that some are often extremely active, while others are at the same time totally insensible.

\* This notion of independence does not destroy the principle of animal organization, that all the parts have a mutual relation; it only indicates that the action of one organ does not absolutely cause the same degree in another. B.

Thus.



Thus in old age, and sometimes in diseases, madness for example, many faculties are lost whilst others subsist; and constant employment of the same faculty sensibly diminishes its energy; if we pass to another we find it has all the force of which it is susceptible, and when we again employ the first faculty, we find it has recovered its original vigour. Thus it is that when fatigued with an abstract and philosophical reading, we turn with pleasure to poetry, and then afterwards apply again with equal attention to our former employ.

These phenomena prove that the faculties are separated and independent of one another, and we are induced to believe that the same is the case with their material organs.

*Note.* "We do not entirely adopt this notion of Gall, but <sup>Theological</sup> on the contrary, we believe that the separation of the material <sup>note.</sup> organs must be considered as the cause of the distinction of the internal faculties; at least it seems to us, that by supposing them originally separated, we cannot avoid the snare of the materialism which exists as soon as we cease to consider the spirit (l'esprit) as unity."

3d. *The Developement of the Organs contained in the Cranium, is in direct Proportion with the Force of their corresponding Faculties.* The organs of the brain are more developed the stronger the correspondent faculties,

This principle dictated by analogy, depends on the axiom; that through all nature the faculties are found to be ever in proportion to their relative organs, and its truth is eminently proved by the particular observations of Gall.

It must however be observed, that exercise has a considerable influence on the force of the faculties, and that an organ but moderately developed, but which is often exercised, may afford a superior faculty to that which accompanies an organ of great magnitude, but is never put in action. Thus we see men whose structure is but moderately strong, acquire by continual exercise powers superior to others whose structure is almost athletic. and the more from exercise.

*Note.* I must here anticipate an opinion which seems to result immediately from this principle, but which is nevertheless false, that the volume of the brain is in direct proportion to the energy of its faculties. Observation has demonstrated to Gall, that the power of the faculties can only be appreciated by the developement of the organs separately, which A large brain does not indicate energy of faculties unless the organs be separately developed.

form

form distinct eminences on the cranium, and that a cranium perfectly round, whatever may be its magnitude, never exhibits faculties either great or numerous.

Conjecture as to the reason.

I do not recollect that I ever heard Gall give his reasons for the conclusion: "but I think we may consider these heads in a state similar to that of obesity, and as we do not judge of the muscular strength of a man or animal by the volume of its members, but by their particular developement, so I think we must judge of the force of the faculties by the developement of their respective organs."

The external figure of the cranium exhibits the organs and shews the power of the faculties, because the form of the cranium depends on that of the brain.

Lastly, the fourth principle which is the most important in the practice of the system of Gall, is: *That we may judge of these different organs and their faculties by the exterior form of the cranium.*

The truth of this principle is founded on another; namely, that the formation of the cranium depends on that of the brain, a truth generally known and proved by the anteriority of the brain, and by the impressions or indentations within the cranium.

Remark.

*Note.* It is true that there are craniums in which there is an internal protuberance of the bone corresponding with the external projection; and this irregularity, which is sometimes found to be a disorder, most commonly in an advanced age, when the organs of the brain do not so powerfully resist the cranium, throws a degree of uncertainty in the practice of the system of Gall; but this is the fate of all our truths which are dictated by experience: these cases, however, are not very frequent.

On these principles Gall has compared the organs of men and animals.

Guided by these principles Gall has examined nature; he has compared the craniums of men and animals, and those of men of similar and of different faculties. His researches have almost incontestibly proved, not only the truths above stated, but that the faculties of animals are similar to those of man; that what we call instinct in animals is also found in man; for example, love, cunning, circumspection, courage, &c. that the quantity of organs is the standard which fixes the generic difference of animals; their mutual proportion, that of the individuals; that the disposition to any faculty which is originally given by nature, may be developed by exercise and favourable circumstances, and sometimes by disorders; but that it can

never be created if not given by nature \*; that the accumulation of organs is constantly made from back to front, and from below upwards, so that animals in proportion as they resemble man in the quantity of their faculties, have the superior and anterior part of the cranium more developed; and lastly in man, the most perfect of animals, there are organs in the superior and anterior parts of the frontal and parietal bones assigned to faculties which exclusively belong to him. "In this last point of view it is that the discoveries of Gall perfectly coincide with the theory of the facial angle, which appears to confirm their truth."

It is difficult to give an accurate and satisfactory account of the detail of this system, and of the various organs which Dr. Gall has discovered, without stating the variety of facts and examples which he exhibits as evidence to prove what he advances; I will however undertake this enumeration, being persuaded that it will in many respects shew the author's manner of reasoning, and give a clear notion of the manner of proceeding required to obtain his results †.

Detail of the system.

### 1. The Organ of Tenacity of Life.

The first organ which the author thinks he has discovered is that of the tenacity of life, *tenacitas vitæ*; he supposes the *medulla oblongata* to be its seat; and as the circumference of the great aperture of the *occiput* is in direct proportion to the extent of the *medulla oblongata*, he judges of the intensity of the life of an animal by the magnitude of this perforation.

Organs.

1. Tenacity of life.

The observations which support this opinion are, that the perforation is generally larger in the *cranium* of women than men; that it is constantly of greater extent in the cat, the otter, the beaver, the badger, &c. animals which are known to be extremely tenacious of life. And the most speedy method of killing an animal is to divide the spinal marrow.

### 2. Organ of Instinct for self-preservation.

Forwarder than the *medulla oblongata* at the place where it quits the brain, the author supposes the organ of love of life, or instinct for self-preservation to exist.

2. Self-preservation.

\* It is necessary that the germ of any organ should subsist in the embryo, in order to its subsequent developement as an organ.

† Compare the corresponding numbers with those of Plate X.

As



As animals do not afford examples of suicide, he could only obtain proofs of this supposition in the human race, and several cases of voluntary suicide in which this part of the brain was disordered, have induced him to consider it as the organ of this faculty; he does not however admit it as indisputably established, but waits for further examples to confirm the fact.

### 3. *Organ of the choice of Food.*

#### 3. Choice of food.

The author supposes the organs for the choice of food to be placed in the quadrijugal tubercles, the anterior of which are greater in carnivorous animals, the posterior developed in the herbivoran, but are of equal size in the omnivorous animals.

### 4. *Cerebral Organs of the external Senses.*

#### 4. External senses.

The middle part of the base of the brain is appropriated to the external senses. It is the region from which those nerves issue which are distributed into the organs of those senses.

(*To be continued.*)

## SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

*Account of the Ventriloquism exhibited by M. FITZ-JAMES.*

#### Account of ventriloquism.

THE reader will doubtless recollect some curious facts and observations on ventriloquism by Mr. Gough in our second volume, page 125. That acute philosopher, reasoning from a few facts and stating the want of more, appears inclined to adopt the theory that the art of ventriloquism consists in causing the voice to issue from the mouth only, and uttering it in such a direction, that the hearer may receive the impression of some echo with considerably more force than he receives the original sound. He gives instances of this process, particularly where the sound of a ring of bells appears to change its direction accordingly, as the hearer by moving along, receives it from different reflecting surfaces, while the original sound is interrupted by some obstacle. Whether the echos in a room be at all likely to be so managed would admit of considerable doubt; and without having witnessed any exhibition of this kind in the least to be compared with the surprising narratives we occasionally hear, I have always been strongly disposed

disposed to think that the delusions with regard to the supposed direction of the voice in such cases are not physical but moral; that is to say, they have arisen from something in the nature of the subject, or the position and action of the speaker, with the character, tone, and manner of speaking. On the present occasion I have the satisfaction to give some account of the performance of M. Fitz. James, one of the first masters of this art; who in addition to his very striking powers as a speaker and actor has the candour and liberality to explain the nature of his performance to his auditors. I was present a few evenings ago at a public exhibition, which continues to be repeated at *Dulau's* in Soho-square; and though my account of what I saw and heard cannot but be very imperfect, and far from exciting the surprise which the actual performance produces, it may nevertheless be of utility to establish a few principles, and remove some errors respecting this art.

After a comic piece had been read by *Monf. Volange*, M. Fitz-james, who was sitting among the audience, went forward and expressed his suspicion, that the ventriloquism was to be performed by the voices of persons concealed under a platform which was covered with green cloth. Replies were given to his observations apparently from beneath that stage; and he followed the voices with the action and manner of a person whose curiosity was much excited, making remarks in his own voice, and answering rapidly and immediately in a voice which no one would have ascribed to him. He then addressed a bust which appeared to answer his questions in character, and after conversing with another bust in the same manner, he turned round; and in a neat and perspicuous speech explained the nature of the subject of our attention: and from what he stated and exhibited before us, it appeared that by long practice he had acquired the faculty of speaking during the inspiration of the breath with nearly the same articulation, though not so loud nor so variously modulated as the ordinary voice formed by expiration of the air. The unusual voice being formed in the cavity of the lungs is very different in effect from the other. Perhaps it may issue in a great measure through the trunk of the individual. We should scarcely be disposed to ascribe any definite direction to it; and consequently are readily led to suppose it to come from the place best adapted to what was said. So that when he went to the door

Account of ventriloquism.

door and \* asked "Are you there," to a person supposed to be in the passage, the answer in the unusual voice was immediately ascribed by the audience to a person actually in the passage; and upon shutting the door and withdrawing from it, when he turned round, directing his voice to the door and said, "Stay there till I call you," the answer which was lower, and well adapted to the supposed distance and obstacle interposed, appeared still more strikingly to be out of the room. He then looked up to the ceiling and called out in his own voice, "What are you doing above? do you intend to come down?" to which an immediate answer was given, which seemed to be in the room above, "I am coming down directly." The same deception was practised on the supposition of a person being under the floor, who answered in the unusual but a very different voice from the other, that he was down in the cellar putting away some wine. An excellent deception of the watchman crying the hour in the street, and approaching nearer the house till he came opposite the window was practised. Our attention was directed to the street by the marked attention which Fitz-James himself appeared to pay to the sound. He threw up the sash and asked the hour, which was immediately answered in the same tone, but clearer and louder; but upon his shutting the window down again the watchman proceeded less audibly, and all at once the voice became very faint, and Fitz-James in his natural voice said, "he has turned the corner." In all these instances, as well as others, which were exhibited to the very great entertainment and surprise of the spectators, the acute observer will perceive that the direction of the sound was imaginary, and arose entirely from the well studied and skilful combinations of the performer. Other scenes which were to follow required the imagination to be too completely misled to admit of the actor being seen. He went behind a folding screen in one corner of the room, when he counterfeited the knocking at a door. One person called from within, and was answered by a different person from without, who was admitted, and we found from the conversation of the parties, that the latter was in pain, and desirous of having a tooth extracted. The dialogue, and all the particulars of the operation that followed, would require a long discourse if I were to attempt to describe them to the reader. The imitation of

the

\* The whole performance was in French.



the natural and modulated voice of the operator encouraging, soothing, and talking with his patient; the confusion, terror, and apprehension of the sufferer; the inarticulate noises produced by the chairs and apparatus, upon the whole, constituted a mass of sound which produced a strange but comic effect. Loose observers would not have hesitated to assert, that they heard more than one voice at a time; and though this certainly could not be the case, and it did not appear so to me, yet the transitions were so instantaneous without the least pause between them, that the notion might very easily be generated. The removal of the screen satisfied the spectators that one performer had effected the whole.

The actor then proceeded to shew us specimens of his art as a mimic; and here the power he had acquired over the muscles of his face was fully as strange as the modulations of his voice. In several instances he caused the opposite muscles to act differently from each other; so that while one side of his face expressed mirth and laughter, the other side appeared to be weeping. About eight or ten faces were shewn to us in succession as he came from behind the screen, which together with the general habits and gait of the individual totally altered him. In one instance he was tall, thin, and melancholic; and the instant afterwards, with no greater interval of time than to pass round behind the screen, he appeared bloated with obesity, and staggering with fulness. The same man at one time exhibited his face simple, unaffected, and void of character, and the next moment it was covered with wrinkles expressing slyness, mirth, and whim of different descriptions. How far this discipline may be easy or difficult, I know not, but he certainly appeared to me to be far superior to the most practised masters of the countenance I have ever seen.

During this exhibition he imitated the sound of an organ; the ringing of a bell, the noises produced by the great hydraulic machine of Marli, and the opening and shutting of a snuff box.

His principal performance, however, consisted in the debates at the meeting of *Nauterre*, in which there were twenty different speakers, as is asserted in his advertisement; and certainly the number of different voices was very great. Much entertainment was afforded by the subject, which was

taken

Account of ven- taken from the late times of anarchy and convulsion in France ;  
triloquism. when the lowest, the most ignorant part of society, was called upon to decide the fate of a whole people by the energies of folly and brute violence. The same remark may be applied to this debate, as to the other scene respecting tooth-drawing ; namely, that the quick and sudden transitions, and the great differences in the voices gave the audience various notions, as well with regard to the number of speakers, as to their positions and the direction of their voices.

This account of a very celebrated ventriloquist may perhaps seem too minute and particular for a philosophical Journal, which is less devoted to do justice to the talents of men, than to investigate the causes of things. But where a striking delusion may lead to mistaken theories of sound, I conceive it to be no small part of the argument, which is to set the truth in a clear light, to shew how numerous and extraordinary the acquisitions and the ability of the performer may be. There is likewise a point of delicacy arising from the suspicion of improper motives, when a public performer is spoken of in terms of approval by a periodical writer. On this head, however, I feel so little difficulty, that I am almost in doubt whether I should obliterate the last observation, or suffer it to pass.

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*Ascent of Mont Blanc and Mont Perdu.*

Ascent of Mont  
Blanc by M.  
Forneret and  
Dortheren,

M. Forneret of Lausanne, and the Baron de Dortheren, have undertaken a new excursion to the summit of Mont Blanc. After two days travel, they arrived at the summit, where the wind was so turbulent that they were forced to sit together with their guides in a mass to prevent their being precipitated. The cold was six degrees below congelation, and with the rarity of the air affected their lungs in so painful a manner, that they declare that no inducement should prevail on them to repeat this expedition. The enterprize was without any beneficial consequence to the sciences. *Bibliothèque de Sonini.*

by de Saussure,

of Mont Perdu  
by Ramond.

It was in the year 1787, that the celebrated De Saussure, arrived at the summit of Mont Blanc ; and his voyage was considered as a real conquest for the natural sciences. Sonini has learned by a private letter, that the indefatigable Ramond, well known from his researches in natural history, has at last reached the summit of Mont Perdu, the giant of the Pyreneans,



means as Mont Blanc is that of the Alps. This mountain covered with glaciers and eternal snows, which is elevated more than eighteen hundred toises above the level of the sea; and rises higher than all the granitic Pics except Mont Blanc, must nevertheless be ranked among ternary mountains, since it contains the remains of sea animals and quadrupeds. The efforts of Ramond to reach the summit have till now been constantly unsuccessful, and he is the only one who has yet accomplished it. It presents on all sides threatening projections and steep precipices. This time, instead of directing his course from north to south by ascending the mountain on the French side, he travelled from south to north by the slope which is directed towards Spain. He will soon publish the interesting recital of his ascension.

#### *Experiment on Sound.*

In the Journal last quoted, there is an account or notice of an experiment performed in the palace of the Tribunal, which as there is some obscurity probably owing to the conciseness of the narrative, I translate literally.

Two figures of the size of life placed at the extremity of a very extensive apartment, hear speaking with a very low voice in whatever position and however distant the persons who speak to them may be, and they answer all questions in the most satisfactory manner.

This curious experiment proves to philosophers, that it is possible to assist the organ of hearing in the same manner as instruments have been contrived to assist the sight.

The author affirms, that it is possible to transmit intire discourses uttered in a low voice to very remote distances, without any indication of the place of the speaker.

#### *Evaporation of Water at an elevated Temperature.*

M. Leindenfroft, in a dissertation published in 1756, announced, that water loses the quality of evaporating accordingly as the heat of the bodies upon which it falls, is augmented from the point of ebullition to the temperature of a white heat. A drop of water which he let fall into an iron ladle heated to whiteness, was first divided into several small globules which afterwards united in one. Considering this with attention, he observed, that it turned with great swiftness



Experiments on  
evaporation.

ness on its axis, and became smaller and smaller. After 34 or 35 seconds it disappeared with noise. A second drop which fell into the same ladle now somewhat cooled, disappeared in nine or ten seconds; lastly, a third disappeared in three seconds. When the spherule of water in the white hot ladle was touched with a cold body, it disappeared in an instant.

Klaproth has repeated these experiments in the following manner. He took a highly polished iron ladle which he heated to whiteness, and threw therein a drop of water, which became divided into several globules of different sizes, and soon united into a single sphere. As soon as this globule which exhibited the phenomena before described, had disappeared, he let fall a second, after which a third; &c. and he observed, that the duration of each was less the more the ladle was cooled. The following is the result of two experiments. In the first experiment the intensity of the heat being greater than in the second, the degree at which the water most speedily evaporates happened later.

1st Experiment.	seconds.	2d Experiment.	seconds.
The first drop lasted	40	The first drop lasted	- - 40
The second - - -	20	The second - - -	- - 14
The third - - -	6	The third - - -	- - 2
The fourth - - -	4	The fourth - - -	- - 1
The fifth - - -	2	The fifth - - -	- - 0
The sixth - - -	0		

These experiments require to be made with much care. M. Klaproth observes, that the slightest circumstances produce variations in the duration of the drops.

Seven drops having been successively thrown into the ladle heated to the necessary degree, soon united into a globular mass, which began its movement by a very rapid rotation. This mass afterwards divided at top, when a spot of white froth was seen at the upper part of the ball, and its edges appeared indented.

This experiment was made with ten drops, and afforded the same result; but when a greater number was employed, the mass was unable to preserve its rotatory motion, and the whole of the water disappeared with a hissing noise.

If instead of the iron ladle or spoon, a capsule of pure silver or of platina heated to whiteness be used, the appearances are nearly the same, but the duration of the balls is commonly longer.

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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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APRIL, 1803

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ARTICLE I.

*An Analysis of a Variety of the Corundum. By the Reverend  
WM. GREGOR. Communicated by the Author.*

THE mineral, which is the subject of the following observations, was presented to me by my valuable and respected friend Philip Rashleigh, esq. who was struck with the peculiarity of its colour. Mr. Rashleigh informed me, that he had found its specific gravity to be 3,6 at the temperature 60, and my experiments gave the same result.

History of the specimen of corundum; from Thibet,

The country where this stone was found, is Thibet: but in what particular part of that extensive region I am not informed.

“The colour of this stone is mottled: the prevailing tint is a diluted lilac. The mass appears to be a confused crystallization, the parts of which are unequal, but mostly very minute. The larger grains reflect a lustre not inferior to that of the common adamantine spar. Although the adhesion of the parts of the mass is not considerable, the parts themselves have a great degree of induration, and from the same cause, probably, a greater specific gravity than the mass itself,” which I found to be 3,603.

Characters,

## A.

Analysis, pulverization, ignition, trituration.

100 grains of this stone, which had been reduced to a powder in a steel mortar, were not affected in the slightest degree by the magnet.

The powder was of a lilac inclining to a pink-colour. Exposed to a strong red heat for half an hour, it lost  $\frac{1}{5}$  of a grain in weight. Its colour was unaltered.—It was now rubbed (in a dry state) to a finer powder in a mortar of flint. Upon weighing it again, I found that it had gained no accession to its weight from the abrasion of it.

Boiled to dryness in potash; then washed; and the residue again treated and washed; and again, &c. till little remained.

It was now put into a silver-crucible, and covered with a solution of potash in \* alcohol, mixed with an equal bulk of distilled water, and the crucible was placed in a sand-bath, and the fluid gradually evaporated; and at last it was boiled to dryness. The salt which remained at the bottom of the crucible, was dissolved in distilled water, and its solution poured off from a spongy earth, and a portion of the undecomposed stone which had subsided. This operation was repeated in the same way with fresh portions of potash, until the whole of the stone was decomposed, except a small quantity of a spongy earth, which was thrown upon a filtre and washed with distilled water, until what passed through it ceased to cause the least turbidness in a solution of nitrat of mercury. The edulcorating water was added to the solution which had been effected by potash.

## B.

The undecomposed residue was partly dissolved by sulphuric acid.

The powder which remained on the filtre was dried. It was of a greyish white colour. I put it into a small matrafs and moistened it with distilled water, and then dropped some

\* I have reason to subscribe to the opinion of Mr. Chenevix, which he has given us in the masterly and scientific paper on the arseniats of copper, with which he has enriched the annals of mineralogy and chemical analysis, that it is by the means of alcohol alone, that potash can be prepared, which is fit for delicate experiments. I was induced to employ a solution of potash in alcohol, because it has been said, that during the process for obtaining it in a dry state, it acts in some degree upon the silver vessels, in which its solution is evaporated. By evaporating it *in contact with the spar*, I thought that this would be less likely to happen.

rectified



rectified sulphuric acid upon it, and placed the vessel in a digesting heat. The powder became at first gelatinous. But this inconvenience was gradually removed, by the further addition of acid. What was dissolved thereby was extracted by distilled water.

The earth which remained, after having been sufficiently edulcorated, was dried and exposed to the action of potash in a silver crucible as before, and what the potash dissolved was added to the alkaline solution before mentioned (A b). What was undissolved by it was heated again with sulphuric acid, until it ceased to act upon it. The soluble part was extracted by distilled water. The residuum was thrown on a filtre and washed with distilled water till it ceased to affect nitrat of mercury: dried and heated red hot for half an hour, it weighed  $\frac{1}{8}$  gr. It was pure filix.

The solution effected by sulphuric acid, was mixed with the edulcorating water and decomposed by ammonia. A white spongy earth was precipitated. When the ammonia ceased to cause any further precipitation, the clear decanted fluid was assayed with carbonat of ammonia and carbonat of potash, but no change was produced on it by the solution of either of those salts. From which circumstance the absence of lime may be inferred.

The precipitate produced by ammonia was sufficiently washed with distilled water, and in a moist state, was boiled with the solution of potash in a silver crucible as before. The alkali dissolved a portion of it. Which solution, together with the edulcorating water, which washed the remainder, was added to the alkaline solution before mentioned, (A b). What remained was thrown on a filtre, and edulcorated as before: dried and heated red hot for ten minutes, it weighed four grains.

It was of the colour of pounded resin. Exposed to the flame of the blowpipe or charcoal it was unaltered. Moistened with melted tallow and ignited, it was not attracted by the magnet. It was taken up by a globule of the phosphat of ammonia and soda, and suspended in it in white flakes. It is soluble in the three mineral acids. But the solution, on being boiled, becomes turbid from the separation of a large portion of it. It is precipitated from its solution in acids by

prussiat \* of potash, of a beautiful grass-green, and by tincture of galls of a dark orange-colour. It possessed, in short, all the properties of the oxide of titanium.

## C.

The alk. solutions sat. with mur. acid, and dissolved; precip. with carb. of potash; andedulcorated.

Dry precip. treated with sulph. acid left filix.

This sulph. solution with acet. of potash gave alum.

The alum. was decomp. by carb. ammon.

The alkaline solution (A b) was saturated with muriatic acid. A copious precipitation took place of a white earth, which was redissolved by a further addition of acid. The contents of this solution were thrown down by a solution of carbonat of potash. This precipitate collected on a filtre, was washed with distilled water, till it passed through it tasteless.

It was dried and introduced into a matrafs, and rectified sulphuric acid, diluted with distilled water, dropped into it, and the fluid was evaporated. Fresh portions of acid were abstracted from it, as long as it seemed to act upon it. The soluble part was extracted by distilled water, and the residuum was edulcorated with distilled water, till it ceased to produce any change upon nitrat of mercury; dried, and heated red hot for half an hour, it weighed 10 grains. It was pure filix.

The solution which the sulphuric acid had effected, mixed with the water with which the residuum had been edulcorated, was gradually evaporated, and a sufficient quantity of acetite of potash was added to it. Regular crystals of alum were formed to the last. A small portion of filix, amounting to about  $\frac{1}{2}$  a grain, was separated.

Distilled water was poured upon the alum, in a quantity sufficient to dissolve it in a warm state; and whilst warm, the solution was decomposed by a solution of carbonat of ammonia. The spongy earth which was separated, was collected on a filtre and edulcorated with distilled water, till it produced no effect upon nitrat of mercury.

\* The prussiat of potash which I employed, was prepared by dropping a solution of tartrite of potash into a solution of prussiat of lime, as long as any tartrite of lime is precipitated. Large and beautiful crystals of prussiat of potash are produced by evaporating the fluid, which I have found to remain unaltered for several years. By this method the intrusion of the sulphuric acid is prevented: but Mr. Henry's late ingenious process seems to be, for many reasons, preferable to it. The prussiat of titanium promises I think, to be useful in the arts. Painters in oil and water colours would find in it a valuable acquisition, as a beautiful transparent green.

The

The alumina was dried gradually, and heated in a crucible, so that the bottom of it was only of a low red : it weighed 111½ gr. Exposed to a strong red heat for half an hour, it = 87¾. Suspecting that a small portion of alkali was retained by it, I rubbed it to a fine powder, and digested it with acetic acid, for many hours, and saturated it with ammonia.

The alumina sufficiently edulcorated, dried and exposed to a strong red heat for half an hour, now weighed 81¾. 100 grains of the original pounded spar, from which I had taken the 100 grains on which I operated, were exposed to the same heat, and for the same time, and lost ⅙ of a grain, as I had found before.

The proportion, which the several ingredients of this stone bear to each other, according to the foregoing analysis, will be,

Alumina - - - -	[C d] - - - -	81,75
	[B b] 1,625	} - - 12,125
Silex - - - -	[C b] 10	
	[C c] 0,50	
Oxide of titanium -	[B d] - - - -	4
Water - - - -	[A a] - - - -	0,937
Loss - - - -	- - - -	1,188
		<hr/> 100

I have repeated the analysis of this mineral \* several times, and have always found it to contain the same ingredients; though there has been a slight variation as to the relative quantities of each. This we might expect to be the case, not only from the loss inseparable from the operations to which the stone is subjected, but also from the nature of the stone itself, as it is not perfectly homogeneous. In one instance, I found the alumina amount to 84 per cent. and the silex proportionably diminished.—Until we have better means than we have at present, of ascertaining the real quantities of water and oxygen which the earths and the oxides of metals severally retain in

\* I am indebted to the kindness of Mr. Rashleigh, for another specimen of corundum from the same place, which differs in some respects in its external appearance from that which I examined. Its colour is not of so bright a lilac, and it seems of a more compact and uniform grain. Its surface is covered with a coat of yellow mica.—This I have not subjected to experiment.

the



the natural state in which they exist, as ingredients of complex stones, we must consider the result of chemical analysis, but as an approximation towards the strictness of truth.

As the intrusion of the oxide of titanium into the place, which has been usually occupied by the oxide of iron, constitutes this stone a variety of the adamantine genus, I have been induced to record my experiments. I am aware, however, that it may be justly observed, that the oxide of neither metals is essential to the nature of the corundum.

Oxide of titanium often met with.

I have no doubt, but that the oxide of titanium will be found to be more plentifully scattered abroad throughout the mineral kingdom, than it has been hitherto imagined. I have detected it in a species of shirl, which occurs in a large crystallized mass, on the tenement of Botallack, in the parish of Just, in this county. It composes several alternate beds or floors of varying dimensions.—The compactness of these beds not having allowed the crystals to shoot with freedom in any direction; It is rarely that they occur with their terminations complete.—These, however, in no respect differ from the common form, and vary only in the length of the prism, from  $\frac{1}{4}$  of an inch to an inch.—The colour of this shirl is black.—I have also lately discovered the oxide of titanium in two species of basalt, of the form of large pebbles, which I found near the sea-shore. Whether they are natives, or whether they were dropped upon our coast accidentally as ballast from some vessel, I have not as yet had an opportunity of ascertaining. That they are *real basalts*, I rest upon the authority of one on whom I can safely rely, whose knowledge in mineralogy can be only equalled by his friendly disposition to communicate it. I mean John Hawkins, Esq.

My experiments also proved, that they contained all the known ingredients of basalts, with the addition of the oxide of titanium.

Creed, near Grampound, Cornwall,

March 2, 1803.

Letter

## II.

Letter from Mr. WILLIAM HENRY. Concerning the Invention of Aromatic Vinegar.

To Mr. NICHOLSON.

SIR,

I AM sensible that an apology is necessary for intruding on you and on your readers, a subject which may appear, on first view, to have little claim to general attention. If the rights and interests of an individual were alone involved on this occasion, I should not have requested a place in your Journal, for the following statement. But it is surely matter of general concern, that the appropriation of discoveries and improvements should be dealt with strict justice to their authors: for the prospect of this distribution of "honour where it is due," is one of the most animating principles of action; and the extinction of this motive would follow an indifference on the part of the public, to the claims of inventors.

The appropriation of discoveries is matter of general concern.

More than fifteen years ago, during the delivery of a course of chemical lectures by my father, in this town, he had occasion to notice a quality of the acetic acid or radical vinegar, which had not to his knowledge been observed before; viz. its property of dissolving camphor and various essential oils. The compound was found to possess a most pungent and agreeable odour; and as the *vinaigre des quatre voleurs* had gained much reputation in preventing infection, it occurred to him that the newly discovered solution would have still more powerful effects, in consequence of its high state of concentration. A bottle of this preparation he gave to a late active magistrate and philanthropist (T. B. Bayley, esq. F. R. S.) who, in the course of an unwearied and undaunted exercise of his public functions, was frequently exposed to the dangers of foul and infected air. Mr. Bayley was highly gratified by its effects; and not only made constant use of the aromatic vinegar on the bench, and on his visits to the prison, but introduced it to the adoption of several of the judges, and principal gentlemen at the bar. He also first suggested to my father the propriety of benefiting by his discovery, and was the medium of a connection with Mr. Bayley, perfumer, in Cockspur Street, which has been continued to the present day.

Discovery that acetic acid dissolves camphor and essential oils.

The

The sanction of an eminent physician given to a subsequent claimant,

The aromatic vinegar, like all other articles in general demand, has been a frequent subject of imitation. But it is not of this that I complain; for in consequence of unremitting attention, our preparation has maintained a decided superiority over all others, both as to quality and extent of sale. The occasion of this appeal to your readers is, that one of these imitations has been sanctioned by the name of a respectable physician, who, though not expressly yet by implication, confers on another the credit of that invention, which in justice is due to my father. (*See a letter from Dr. Trotter, physician to his majesty's fleet, contained in the advertisement of a London drug-gift.*)

Subsequent proceedings, &c.

From the recommendatory letter alluded to, it is sufficiently evident that Dr. Trotter was unacquainted with any prior claim to the invention of the aromatic vinegar; and he was therefore furnished by my father, in the most respectful manner, with the facts that have been laid before you. To this letter the doctor has never replied, though he declared verbally, to a medical gentleman, that my father's preparation had never fallen in his way; and that if it had, he should with equal readiness, have given testimony in its favour. The advertisement, however, still continues to be regularly inserted; and I therefore deem it expedient to appeal thus publicly, against the injustice of such a proceeding; especially in behalf of a man, who has imitated the original, only in assuming, with the coolest effrontery, an advertisement drawn up by myself.

I believe there are few of your readers, who will not decide, that the ordinary forms of civility required Dr. Trotter to have taken some notice of the letter that was addressed to him; that such an attention ought to have been paid to one of the oldest practitioners of medicine in this country; and that more respect was due to a man (whom I trust it is not unbecoming me to characterize, in terms already publicly applied to him, *viris laudatis* \*) "respectable in science and literature," and "distinguished by ingenuity, honour, and the strictest integrity."

I am, Sir,

your obliged friend and servant,

WILLIAM HENRY.

Manchester, March 13, 1803.

\* Dr. Aikin and Dr. Percival.

Caution



## III

*Caution against the Danger of leaving Phosphoric Preparations in the Vicinity of Wood. By a Correspondent.*

To Mr. NICHOLSON.

March 1, 1803.

S I R,

ACCIDENTS in chemical experiments not feldom afford useful facts or suggestions ; but I am doubtful whether or no the following occurrence will be thought worth notice in your Journal : however, the account of it is at your service.

While at lecture this morning, we were suddenly annoyed by a column of white fumes issuing from amongst bottles of preparations on a shelf, which was soon followed by flame. On examination I found the inflammation proceeded from a bottle containing lime, into which phosphorus had sublimed in the upper part of a tube in the making of phosphuret of lime. This was not therefore phosphuret of lime, but only the mixture of particles of ignited phosphorus, with pulverized lime scarcely ignited. In this circumstance the phosphorus I know becomes oxygenized to be in the state of *oxidule*, if not of *oxide*; which composition it is well known, I suppose, is employed for the charging of little bottles to inflame sulphur matches by mere friction of them within side of the bottle. In the present case, the bottle containing the lime with phosphorus had cracked, so as to admit air, which excited the inflammation at the temperature of about 65°. Such preparations should therefore be kept out of the way of wood, or any inflammable substance, for if this accident had happened when no one was present, the whole laboratory would have been set on fire.

Narrative of fire  
occasioned by  
oxidule of phos-  
phorus.

Your's truly,

Z,

IV. Description

## IV.

*Description of a new Reflecting Quadrant. By Mr. EZEKIEL WALKER. From the Author.*

To Mr. NICHOLSON.

SIR,

Observations on  
Hadley's qua-  
drant.

THAT instrument which goes by the name of Hadley's Quadrant, may perhaps be deemed one of the most useful inventions of the last century. Although its arc is only the one eighth part of a circle, yet it is so constructed as to measure angles from  $0^{\circ}$  to  $90^{\circ}$  by the fore observation, and from  $90^{\circ}$  to  $180^{\circ}$  by the back observation. But as no method has yet been found out, by which the back horizon glass can be adjusted with the same exactness as the fore one, all angles above  $90^{\circ}$  which are taken by the back observation, cannot be relied on so much as those that are taken by the fore observation. To obviate this inconvenience, the sextant was invented, by which any angle less than  $120^{\circ}$  may be taken by the fore observation. This is an invaluable instrument to the nautical astronomer; and indeed it is much to be regretted, that its use is not so generally understood by travellers, as the imperfect state of geography requires. It must, however, be admitted, that it is not quite free from imperfections. First, an angle greater than  $120^{\circ}$  cannot be taken by it; and secondly, when a large angle is taken, the rays of light fall so obliquely upon the index glass as may occasion some doubt respecting the truth of the observation. Mr. Ludlam gives it as a general rule, in constructing an octant, "that very oblique reflections from the mirrors ought to be avoided\*." If it be necessary to observe this rule in constructing an octant, the same should be attended to, as much as possible, in taking observations.

Imperfections of  
the sextant.

New instrument  
measuring to  
 $180^{\circ}$  without  
oblique reflec-  
tion.

These imperfections suggested the idea of an instrument to measure any angle between  $0^{\circ}$  and  $180^{\circ}$  by the fore observation, without very oblique reflections from the mirrors. Plate XII. Fig. 2. exhibits a plan of this instrument, where L M C represents an octant, constructed in the usual way, A B the

\* Ludlam on the use of Hadley's quadrant, par. 91.

index glass,  $m$  the horizon glass, and  $HE$  the line of sight, or axis of the telescope. Upon this line of sight, at an angle of 45 degrees, let another horizon glass be fixed, represented by  $xy$ , which may be called *the second horizon glass*.

When a ray of light  $Rn$  falls upon the centre of the glass  $xy$ , at an angle of 45 degrees, it will be reflected to the eye at  $E$ ; because the angle  $xnE$  (the angle of reflection) is  $= 45^\circ$  by construction, and consequently  $RnH$  and  $RnE$  are right angles.

*The Use of the Reflecting Quadrant in taking Angles.*

All angles less than  $90^\circ$  are to be taken by this instrument, <sup>Manner of using it.</sup> in the same manner as by the sextant. But, suppose it were required to observe the supplement of the sun's meridian altitude at sea, let the axis of vision be directed to the zenith, and turn the quadrant till that part of the horizon, which is opposite to the sun, be seen by reflection from the second horizon glass; then turn the index from  $O$  towards  $M$ , till the sun appears in the telescope, with one of its limbs in contact with the horizon, and the index will shew the sun's observed zenith distance; to which add  $90^\circ$ , and the sum will be  $=$  the angle observed by the quadrant,  $=$  the observed supplement of the sun's meridian altitude.

*To adjust the Second Horizon Glass.*

Let the arc  $LN$  be made equal to the arc  $LM$ , and graduated from  $0^\circ$  at  $L$ , to  $90^\circ$  at  $N$ . Then, after the index error has been determined, set the index to  $90^\circ$  at  $N$ , and elevate the telescope till the sun, or some other remote object, be seen by reflection from the index and horizon glasses, and the same object will also be seen by reflection from the second horizon glass: consequently, the angle  $RnH$  becomes known to as great a degree of precision as the index error.

I am,

SIR,

Your obedient servant,

EZEKIEL WALKER.

*Lynn Regis, Feb. 20, 1803.*



## V.

*Miscellaneous Information. Mistake respecting Dr. Thomson, Author of the Elements of Chemistry. Observations on the Scottish Querns. On the supposed Determination of the real Zero of Heat. In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

Mistake respecting Dr. Thomson. I BEG leave to observe a small inaccuracy in the notice \* of Dr. Thomson's System of Chemistry. Thomas Thomson, M. D. Lecturer on Chemistry in Edinburgh, who is much esteemed for his extensive knowledge, is the author of that excellent work. The Notes on Fourcroy's Elements of Chemistry were written by Mr. John Thomson, surgeon in Edinburgh.

The following remarks are at your service, from

SIR,

Your very humble servant,

II.

Edinburgh, Feb. 17. 1803.

1. *Observations on the Scottish Querns.*

The Scottish quern or hand mill.

The Indian hand mill, described in the Philosophical Journal (III. 186), very much resembles the hand mill or *querns* used by the cottagers in Scotland for grinding malt. There are many varieties of querns, but the greater number of them answer to the description just quoted, to which I beg leave to refer the reader.

The querns in Aberdeenshire have a peculiar form: a cylindrical cavity in the under stone contains the upper one. A plug of wood is driven in the center of this stone for fixing the gudgeon, and there is an eye or aperture in the angle through which the ground malt runs out. Instead of cutting so much out of the solid stone, it would be better to use a wooden drum. It is unnecessary for me to give a more minute detail of the Scottish querns, which have been long known in this

\* Phil. Jour. 8vo. III. 62.

country; several pieces of them having been found in the ruins of very old buildings.

In Dumfriesshire there is a small pair of mill stones on a frame, driven by a crank, a vertical cog wheel, and a horizontal pinion, which grind very well, and are extremely useful. The stones have a drum and hopper like a single mill. An easy expeditious method of reducing grain to flour, is of the utmost consequence to the inhabitants of every civilized country, and ingenious men deserve our best thanks for their reiterated attempts to obtain so desirable an end.

## II. *Observations on the supposed Determination of the real Zero.* On the zero of heat.

Many eminent chemists have endeavoured to discover the real zero, or the division on the thermometric scale corresponding to the total privation of caloric, but their labours have hitherto been unsuccessful. Dr. Irvine of Glasgow contrived a theorem for this purpose, which he founded on suppositions and experiments; and Mr. Dalton has lately proposed an hypothesis for the solution of the same problem. These may be considered in their order.

Without giving Dr. Irvine's theorem, which is so well known, I shall insert the principal results of the experiments and calculations founded on it.

In the following tabular view, the numbers in the first column denote degrees of Reaumur's scale *above* or *below* the freezing point, and those in the second column indicate degrees of Fahrenheit's scale *above* or *below* the same point, accordingly as the sign *plus* or *minus* is prefixed respectively.

Reaumur. Fahrenheit.

M M. Lavoisier and Laplace, from experiments on a mixture of nine parts of water and 16 parts of quick lime, place the real zero at - - -  $-1537^{\circ} = -3458\frac{1}{4}^{\circ}$

Their experiments on a mixture of sulphuric acid and water, in the proportion of 4 to 3 respectively, fix it at -  $-3241 = -7292\frac{1}{4}$

Their experiments on a mixture of the same fluids in the proportion of 4 to 5 place it at - - -  $-1169 = -2630\frac{1}{4}$

And

Reaumur. Fahrenheit.

And the experiments of these celebrated philosophers, on a mixture of nitrous acid and quick lime in the ratio of  $9\frac{1}{3}$  to 1, give for the real zero,

$$\begin{array}{r} 1889 \\ -0.01783 \end{array} \quad - \quad - \quad - \quad - \quad - \quad + 10594\frac{1}{2} = + 23837\frac{3}{5}$$

Seguin, from the experiments of Lavoisier on the combustion of phosphorus narrated in his Elements of Chemistry, determines the place of the real zero to be

$$-842 = -1894\frac{1}{2}$$

He found, by the experiments of Lavoisier on the combustion of carbon, that it should be

$$-1204 = -2709$$

And he inferred, from Lavoisier's experiments on the combustion of hydrogen gas, that the real zero should be at

$$-739 = -1662\frac{3}{4}$$

The comparison of the capacities of water and ice, which, according to the experiments of Kirwan, are as 1 to 0.9, fix the real zero at \*

$$-600 = -1350$$

Dr. Crawford, from experiments on the combustion of hydrogen gas, places the real zero at †

$$-680\frac{3}{5} = -1532$$

Gadolin's experiments on the conversion of snow into water, taking the capacity of snow to that of water as 9 to 10 according to Magellan, place the real zero at ‡

$$-649\frac{3}{5} = -1461\frac{3}{5}$$

These results differ widely, and one is impossible.

These ten widely different results, of which one is impossible, induce us to conclude that one of Dr. Irvine's suppositions at least is erroneous. Indeed, it seems to be agreed that his theorem is not well founded. The seventh and ninth numbers also shew, that a near approximation to accuracy is

\* An interesting Paper by M. Seguin, from which the first eight numbers are taken, may be seen in the Annales de Chimie, V. 255.

† Crawford on Animal Heat, 267.

‡ Ibid. 458.

scarcely



scarcely attainable in these experiments. Let us now consider the second hypothesis.

In the Philosophical Journal, Vol. III. page 130, there is a very important paper of Mr. Dalton's on the Expansion of <sup>Mr. Dalton's hypothesis.</sup>

Elastic Fluids, where he says, "In order to explain the manner in which elastic fluids expand by heat, let us assume an hypothesis, that the repulsive force of each particle is exactly proportional to the whole quantity of heat combined with it, or in other words, to its temperature reckoned from the point of total privation: then, since the diameter of each particle's sphere of influence is as the cube root of the space occupied by the mass, we shall have  $\sqrt[3]{1000} : \sqrt[3]{1325} (10 : 11 \text{ nearly}) ::$  the absolute quantity of heat in air of  $55^{\circ}$  : the absolute quantity in air of  $212^{\circ}$ . This gives the point of total privation of heat, or the absolute cold, at  $1547^{\circ}$  below the point at which water freezes."

According to this hypothesis, I have computed the following numbers, where the same is to be understood as in the last table, except the degrees in the first column, which are those of the centigrade thermometer. Computations of zero made from the same.

	Celsius.	Fahrenheit.
$\sqrt[3]{1000} : \sqrt[3]{1325} :: 10 : 10.983446,$		
which places the real zero at	- - - $-874^{\circ}.12 = -1573^{\circ}.43$	
$\sqrt[3]{1000} : \sqrt[3]{1375} :: 10 : 11.1199,$		
which fixes it at	- - - $-892.93 = -1607.29$	
$\sqrt[3]{1167} : \sqrt[3]{1325} :: 10.528 : 10.9834,$		
nearly, which gives	- - - $-952.34 = -1714.2$	
$\sqrt[3]{1000} : \sqrt[3]{1167} :: 10 : 10.528268,$		
and the real zero is at	- - - $-827.77 = -1485.98$	

These deductions do not differ nearly so much as those in the first table; yet they give grounds for believing that the supposition, or the greater part of the experiments is erroneous. These results apparently of little value. It is no support to the validity of this hypothesis to say, that the first number nearly agrees with one deduced from a method totally different; for it might have been considerably greater or less than it is, and yet have been nearly equal to one of those in the first table. There seems to be a slip in the degrees mentioned in Mr. Dalton's paper; for  $167 + 158 = 325$ , the whole dilatation between  $55^{\circ}$  and  $212^{\circ}$ , while  $77\frac{1}{2}^{\circ}$

$+77\frac{1}{2}^{\circ} = 155^{\circ}$  only, which should be  $157^{\circ}$ , of which the half is  $78\frac{1}{2}^{\circ}$ . This might easily happen: in the calculations of the third and fourth number I have used  $78\frac{1}{2}^{\circ}$ .

Mr. Dalton and Cit. Gay Lussac have considerably improved the method of experimenting on gases, and it is to be hoped they will push their improvements still further.

II.

## VI.

*Outline of the Craniognomic System of Dr. Gall of Vienna. By Dr. BOJAMES.*

(Concluded from Page 202.)

5. *Organ of the Instinct of Sexual Union,*

5. Sexual union.

THE organ of the instinct of sexual union is situated at the base of the *occipital*, behind the *medulla oblongata*, and the large aperture of the *occipital*.

This organ is not developed until the age of puberty; and it in a great measure influences the figure of the back of the neck, because the muscles are attached to this part of the *cranium*.

The developement of this organ does not take place in animals which are castrated before puberty, for which reason it is invariably observed that bulls have much stronger necks than oxen, and "that horses which have undergone this operation, before the neck is formed, are always slender in that part."

In the monkey, the hare and the cock, this organ is very distinct, and in pigeons and sparrows the *occiput* forms a peculiar sack, which appears to be an appendage to the head; it is also known that these animals have a strong disposition to copulate. The same configuration is sometimes found in the *cranium* of men, and Gall possesses in his cabinet several skulls of idiots, who were noted for lasciviousness, in which the *occiput* has a very unusual projection.

6. *Organ of the Mutual Love of Parents and Children.*

6. Parental and filial affection.

The organ of the mutual love of parents and children occupies all the back and upper part of the *occipital*; from its position

tion it is intimately connected with the preceding organ, which consequently influences this by its action. "The excessive development of this organ sometimes contributes to form that prolongation of the *occiput* of which we spoke in the preceding article."

This organ is in general more distinct in women than in men, and throughout nature it is more defined in the female than the male; it is more particularly apparent in monkeys, whose fondness for their young is so remarkable as to have become proverbial.

"In general, all those animals which shew much affection for their young are provided with it; and it appears to us that pigeons, the male of which sits on the eggs as well as the female, and which feed their young nestlings by a kind of rumination, may be taken as an example."

The cuckow, which never rears its young, is almost entirely destitute of this organ.

#### 7. Organ of Attachment, of Friendship.

Behind and between the parietals, and on the lateral parts of the *occipital*, is placed the organ of attachment, or of friendship. 7. Attachment; friendship.

"By its position it has an intimate connection with the two preceding organs, and it is in animals destined to live in societies that the united action of these three organs takes place."

Dogs shew the most astonishing marks of attachment, and spaniels, terriers, and house-dogs afford the greatest number of examples; these species are also distinguished by a large head, in which the development of this organ is found behind and above the *zygomatic apophyses*. The head of the greyhound, which is less susceptible of attachment, is narrower behind, and usually without this organ.

#### 8. Organ of Courage.

The organ of courage is placed at the posterior and inferior angle of the parietals. It assists in increasing the size of the head and separating the ears from each other. Its proximity to the three preceding organs accounts for the fury of animals in rutting time, and for the extraordinary courage of those which have young, or which protect their females or the individuals of their society. 8. Courage.



It is most distinct in the hyena, the lion, the wolf, some species of dogs, and particularly in the wild boar, the fierceness of which is well known.

On the contrary, the ass, the greyhound, the sheep, the hare, which are distinguished by their timidity, are destitute of this organ; their heads are narrow behind, and their ears almost close together.

A singular phenomenon seems to give countenance to the opinion of Gall respecting the situation of this organ; it is the involuntary action of a man who loses his courage. He rubs behind his ears, as if endeavouring to stimulate the action of that organ from which this faculty is derived.

*Note*, "We have noticed an action of cats which appears to have some resemblance to the above, and which regards the organ of attachment. It is, that in caressing man they rub against him with the back part of the head."

#### 9. *Organ of the Instinct to Assassinate.*

9. *Assassination.* The organ of the instinct to assassination is situated more forward than the organ of courage, towards the middle of the sides of the parietals.

It is developed in all the carnivorous animals who live by prey; and Gall found it in the skulls of several murderers.

#### 10. *Unknown Organs.*

10. *Unknown.* Two organs which correspond with the temporal bones, are yet unknown as to their functions.

#### 11. *Organ of Cunning.*

11. *Cunning.* The organ of cunning occupies the front and lower parts of the parietals; it is developed in all those animals which are distinguished for this faculty, such are the fox, the pole-cat, the cat, the diver\*, and it is in the most intimate combination with the *organ of theft*, which is only a prolongation of this towards the orbit, and is found in cats, some dogs, and in magpies.

\* An observation which it appears difficult to reconcile to this, is that Gall constantly observed this organ to be developed in poets; he gives no explanation, but his observation is accurate.

It

It is perhaps to the developement of this organ that the Calmucks, whose national character is that of theft, are indebted for that magnitude of their heads which has been noticed by some observers.

### 12. *Organ of Circumspection,*

The organ of circumspection is situated in the middle of the parietals, above the organ of cunning, and that of the instinct to assassination. 12. Circumspection.

The excessive developement of this produces irresolution, and its defect causes heedlessness; it is distinct in the chamois and the rein-deer, the circumspection of which is remarkable, and which do not tread over unknown paths but with the greatest precaution.

It is also found in such animals as only leave their retreats at night, such as owls, otters, &c.

### 13. *Organ of the Instinct for Self-Elevation.*

This organ, in the middle of the inner border of the parietals, a little farther back than the middle of the upper part of the head, gives us a true notion of the difficulties to be overcome in the researches of Gall, and at the same time affords a striking example of the happy opinions of this accurate observer. 13. Self-elevation.

He found this organ well developed in the chamois, and still more so in the wild goat; he also noticed the same in many men distinguished by their pride. It was difficult to bring these observations into one point of view; but on considering that the chamois frequents the most lofty parts of mountains, that the wild goat is constantly endeavouring to ascend higher, and that pride, attentively examined, is only the desire to be superior to others, he was persuaded that this was the organ which produced these effects, *apparently so differing*, and he took it for the organ of the instinct of raising or self-elevation.

The head of the proud man, raised and thrown back, tends to confirm this opinion still more,

*Note,* "It appears to us that the figure of a proud man, opposed to that of a submissive and modest man, renders the truth of this notion more obvious. In the first every thing is directed upwards; he sets up his hair, raises his head, lifts his

his brows, turns up his eyes, throws back his shoulders, walks on tip-toe, and considers every surrounding object as beneath him; in the other, on the contrary, the hair falls naturally, the eyes, the eye-lids and the head are turned downwards, the body and the knees are slightly bent; in short, every thing indicates a state of submission, without a wish for superiority."

#### 14. *Organ of the Love of Glory.*

14. Love of  
glory.

When this organ is extended farther on the sides, it forms that of the love of glory, a propensity very analogous to pride.

#### 15. *Organ of the Love of Truth.*

15. Love of  
truth.

The function of the organ which is seen at the posterior and superior angle of the parietals, is not exactly fixed by Gall; nevertheless he has reasons to consider this angle as the seat of the organ of the love of truth; but he has not yet collected a sufficient number of facts to produce entire conviction.

*Note*, "We have some difficulty to persuade ourselves of the function attributed by Gall to this last organ; it appears to us, that an organ found among those with which animals are provided as well as men, ought not to be appropriated to a faculty, like the attribute of veracity, is adapted only to the latter.

"Nevertheless this faculty, like that of pride, may be capable of great modifications in animals: and we acknowledge that we have known two men, one of whom was distinguished by extreme veracity, and was furnished with this organ in a very eminent degree; but the other, on the contrary, whose disposition for falsehood was extraordinary, was so entirely destitute of it, that instead of a projection, there was a cavity in this part of the head."

In the anterior, or lower part of the *os frontis*, Gall discovered many organs, the functions of which are of great importance.

At the commencement of his investigations, he considered them as organs of the different species of memory; but afterwards observing that their action was not merely reproductive, but also productive, he was induced to consider them as the organs of particular senses, and to establish, from this observation, the opinion that memory in general is only the reproductive operation of all the organs; and that imagination, on the contrary, is their productive action.



The automatic motion of man who endeavours to recollect, seems to relate to these organs. He places his hand, unconsciously, upon the lower part of his forehead. This action, though unobserved by him who performs it, is nevertheless constant, and is never confounded with that mentioned before, under the head of the organ of courage.

#### 16. Organ of the Sense of Locality.

The organ of the sense of locality occupies the fore part of the *os frontis*, which corresponds to the protuberances above the orbits (*protuberantiæ suprâ orbitales*); it is generally found in the craniums of those who are distinguished by large frontal sinuses, and uniformly have an anterior cavity adapted to an elevation of the brain. 16. Sense of locality.

When it acts reproductively, it constitutes what we term local memory (*memoria localis*); by its productive operation, on the contrary, it inclines to combinations of new localities.

It is this organ which, in unknown places, directs the blood-hound, in whom it is strongly defined; it exists in all birds of passage; it incites them to change of place, to take long flights, and to return to their first habitations: the stork and the swallow are eminently provided with it, as well as those animals which remove the farthest from our climates. In such men as are furnished with it, we also discover a strong remembrance of places and a desire to travel; it is also constantly found in good painters of landscapes.

“A general, who regulates the dispositions of an army, and who at a glance must discern all the localities of the country he occupies, cannot succeed without it.” The great Frederic furnishes us with a striking example. In advanced age, this organ is one of those which sensibly diminishes: it is also notorious that every kind of memory and of imagination grow weaker as the individual becomes older; the frontal sinuses then increase within; the action of the brain is no longer capable of obstructing their developement.

#### 17. Organ of the Sense of Things (*sensum rerum*.)

The sense of things has its correspondent organ in the lower and anterior part of the *os frontis*, between and higher than the preceding; its action is both productive and reproductively; and, in the latter case, it gives the remembrance of facts and things. 17. Sense of things

This

This organ is very necessary to education and instruction, which absolutely require the recollection of past circumstances; in age it is subject to the same change as the preceding.

Among animals, the elephant is particularly distinguished by the developement of this organ; this animal also retains with the greatest accuracy those circumstances and acts which have a reference to itself.

“ Among men, we have found this organ, not only in those whose memory of facts and objects was powerful, but also in those which might be denominated systematic heads, from arranging facts in order, and hence forming conclusions; in those who possess a happy conception, and are distinguished by a desire of universal knowledge; it even appears to us, that the operation of combining facts with a view to obtain a result, is one of the principal actions of this organ; at least the elephant, who keeps his trunk filled with water, for the purpose of throwing it over the man who had offended him the night before, arranges a number of facts, and draws from them a result which is a true logical conclusion; and we are not acquainted with any other organ in the elephant to which this action can be referred.

“ The automatic movement of the man who perceives that he has reasoned wrong seems to give effect to these conjectures. He strikes himself on the middle of the forehead.”

#### 18. *Organ of Painting, Sense of Colours.*

18. Painting or colours.

The organ of the sense of colours, or of painting, occupies the fore part of the *os frontis*, above the orbit; Gall has observed this organ in all painters of eminent talent.

“ Having been acquainted with this discovery but a short time, we have not been able to collect many observations on it; we have nevertheless remarked it in some individuals, and it is very apparent in the head of Raphael, in the National Museum, No. 57.”

#### 19. *Organ of the Sense of Numbers.*

19. Sense of numbers.

The organ which corresponds to the lower and exterior part of the *os frontis*, near the zygomatic apophysis of that bone, possesses the function of the sense of numbers; it exists in men whose memory is good with respect to numbers, and in arithmeticians who combine calculations with facility; it exists

exists in a species of magpie capable of counting as far as nine, which is the only example known among animals.

“ We have had an opportunity of noticing this organ on the head of a blind man, at the Quinze-Vingts, remarkable for his arithmetical talents; and Gall preserves the busts of many men which afford very instructive examples.”

## 20. *Organ of the Musical Sense.*

Above this organ is found that of musical sense, or for 20. Music. sounds.

It acts in a manner similar to the other organs, productively and reproductively; it gives the memory for sounds; it facilitates the new combinations of musical composition; it induces birds to sing; it acts upon those who learn to speak, and in whom language is founded only upon this remembrance of sounds.

It is intirely wanting in animals which have no musical sense; it is strongly developed in the parrot and the starling; and those great musicians Gluck, Mozart, Haydn, Pleyel, furnish us with striking examples.

## 21. *Organ of Sense for Mechanics.*

In the lateral and inferior part of the *os frontis* is found the organ of sense for mechanics. The beaver which forms its habitation is eminently provided with it; it exists in the field-mouse and in the birds which make their nests with much art; it is met with in men of mechanical talents, who construct with ease any machine, who use their hands with dexterity, and who excel in the different arts which require manual labour. Though it may be difficult to judge of the existence of this organ, when it is but slightly developed, “because the temporo-maxillary muscle covers this part of the cranium; it is nevertheless very apparent if the faculty exist in a superior degree, and it is then one of those organs respecting which there can be the least doubt.” 21. Mechanics.

## 22. *Organ of Verbal Memory.*

In the interior of the orbit, at the bottom of the upper part, exists the organ of verbal memory; it may be noticed from its developement by the effect it produces on the position of the ball of the eye, which it impels forwards, and more or less out of the orbit. 22. Verbal memory.



Persons provided with it easily retain words by heart. Gall while young, remarked this faculty in several of his school-fellows, who excelled merely by this talent, and who were distinguished by the protuberance of their eyes. This was the first observation which afterwards led him to these investigations. A number of subsequent observations have established the truth of its existence and of its function.

### 23. *Organ of Sense for Languages.*

**23. Languages.** The organ at the exterior and upper part of the orbit, is called by Gall, the organ of sense for languages. Its presence has a considerable influence upon the position of the ball of the eye; it pushes it downwards and towards the nose, and increases its distance from the upper edge of the orbit; it never exists in animals, in whom the ball of the eye is directed more towards the exterior side of the orbit.

Distinguished talents for the languages are invariably attended by its developement; it is eminent in great philologists; and though it may be difficult to decide from external appearances, respecting its existence, we have nevertheless remarked that it never escaped the penetrating eye of Gall, and that he was never mistaken in this point.

### 24. *Organ of the Memory of Persons.*

**24. Memory of persons.**

The function of the organ at the upper internal part of the orbit, has not yet been discovered by Gall; nevertheless many observations on man and animals, such as the dog and the horse, induced him to suppose it the organ of recollection of persons. The developement of it, like that of the preceding, must influence the position of the eye; it should contribute to remove it from the upper edge of the orbit, and to push it towards the external side, if an equal developement of the preceding organ do not counterbalance its action.

### 25. *Organ of Liberality.*

**25. Liberality.**

The organ of liberality is situated in the anterior part of the *os frontis*, above that of the sense of locality, and of the sense for painting (Nos. 16 and 18), and beside that of the musical sense (No. 20); its extreme developement is a concomitant of prodigality; it is not to be found in the miser; in that case, a hollow is formed in this part of the *os frontis*. Gall is in possession of numerous examples.

“ The

“ The proximity of the organ of music, and that of the sense for painting (Nos. 18 and 21) appear often to assist the developement of that of liberality; this is perhaps one reason why we so often find prodigality among men of eminent abilities in these sciences.”

We constantly observe, that as a man becomes old, he becomes covetous; thus in advanced age the diminution of this organ is so remarkable, that its place is frequently occupied by a cavity in the *os frontis*, which is sometimes of considerable size.

#### 26. Organ of the Power of comparing Things.

The organ above the sense of things, in the middle of the forehead, is destined to a faculty which Gall calls the spirit of comparison (*judicium comparativum*). 26. Comparing or combining.

It forms an oblong eminence, and is found in men who avail themselves easily of figures and images in conversation; who are not at a loss for expressions; who narrate fluently; and possess great eloquence.

#### 27. Organ of Metaphysical Talent.

When this organ is more developed towards the sides, so as to form a round prominence, raised in the middle of the forehead, it is the index of metaphysical talent. 27. Metaphysics. Among the busts of ancient philosophers, that of Socrates affords the most striking example; among modern philosophers remarked for this organ, I shall only notice Kant as one of the most celebrated.

Note, “ I recollect in one of my first school-fellows, to whom we had given the name of philosopher on account of his attachment to the abstract sciences; that his forehead presented a very visible developement of this organ.”

#### 28. Organ of the Talent for Observation.

The organ of the talent for observation spreads over the whole of the anterior part of the *os frontis*, and its developement approaches more or less to the front of the vertical line. 28. Observation. It is found on the craniums of all the men of observation in all ages; the celebrated physician Frank possesses it in an eminent degree, and Gall himself is very evidently furnished with it.

#### 29. Organ

29. *Organ of the Talent for Satire.*

29. Satire and wit.

The organ of satire and facetiousness (*witz* of the Germans, *wit* of the English, *facetiae* of the Latins) corresponds with the frontal protuberance. Gall preserves many examples of the truth of this opinion, and we have uniformly found it true.

30. *Organ of Goodness.*

30. Goodness.

The organ of goodness is in the middle of the forehead, above that of comparison (No. 26.) It forms that oblong elevation which is constantly found in the portraits of Christ and of Mary, painted by Raphael and Correggio, and contributes much to that expression of mildness and goodness with which we are delighted; it is always seen in the craniums of men naturally good, and is wanting in those of the mischievous and vindictive\*.

Among animals the roe-buck, the hind, the pigeon, &c. are provided with it; on the contrary, animals of prey, such as the eagle, the starling, the tiger, the fox, &c. are without it; in the latter case, the *os frontis*, instead of being round and elevated, is depressed and hollow.

31. *Organ of Music or of Theatrical Talent.*

31. Theatrical representation.

The very marked enlargement of the summit of the *os frontis*, is owing to the development of the organ for the representation of thoughts by actions, the organ of music, or theatrical talent.

"Gall has collected many observations to prove the truth of this opinion, nor will it be overlooked inattentively, considering the heads of the great performers at the different theatres of Paris."

*Note*, "We think we have also observed that this organ is particularly developed in the deaf and dumb, and we attribute it to the necessity such persons are under of acting continually, an exercise which necessarily facilitates its advancement."

\* We are not here speaking of that goodness which is the result of moral principles, that to which we allude exists as instinct, without being the produce of moral reflections.

32. *Organ*



32. *Organ of Theosophy.*

The organ of theosophy occupies the most elevated part of 32. Theosophy. the *os frontis*.

All the portraits of saints which have been preserved from former ages, afford very instructive examples, and if this character is wanting in any one of them, it will certainly be deftitute of expression.

It is *excessively* developed in religious fanatics, and in men who have become recluse through superstition and religious notions.

It is the seat of this organ, which according to Gall, has determined men to consider their gods as above them in a more elevated part of the heavens. In fact, when we consider this subject philosophically, there is no more reason for supposing that God is placed above the world, than there is to suppose him below it.

33. *Organ of Perseverance.*

The last of the organs hitherto discovered by Gall, is that 33. Perseverance. of perseverance, of constancy, of character; it is situated in the anterior and superior part of the parietals in the middle of the head; when it is in excess it causes obstinacy, but its defect produces inconstancy.

“With regard to those parts of the skull in which Gall has not yet discovered organs, it is probable that his future investigations will afford him the means of success; on this subject, the work which he is about to publish, will give us more ample details. It will also be for him to convince us, by arguments perhaps incontestable, of the truth of his system, the exposition of which cannot be very satisfactory in a cursory outline.”

We think it necessary also to remark, that the organs here enumerated are not distinctly perceptible, except in individuals who possess some faculty in an eminent degree, and that it is not possible to form a correct judgment of a moderate talent, on account of its organ being confounded among those which surround it. “We see no reason, philosophically speaking, for the calumnies which have been lately thrown upon the system of Gall, that *it tends directly to materialism*. When we admit of organs of the action of the internal faculties, the immeasurable space between mind and matter will  
continue

continue the same. Objects of a nature so unlike, are incapable of union. On the other hand, the will of man continues unimpaired; it is this which must counterbalance the operation of the organs, morality ought to subdue the passions."

BOJAMES, M. D.

## VII.

*Method of conveying Boats or Barges from a higher to a lower Level, and the contrary, on Canals, by means of a Plunger, instead of losing Water by Locks. By LAWSON HUDDLESTON, Esq. of Shaftsbury, Dorset. Communicated by the Inventor.*

Construction of  
a lock for raising  
and lowering  
boats.

WHEREVER there is occasion to convey a commercial boat or barge from a higher canal to a lower (usually termed a higher or lower line) or *vice versa*, a lock must be constructed of stone or other fit materials in the space between the higher and lower canal, so as to communicate at the ends with both of them. The dimensions of the lock are to be as follow; its horizontal superficies or area should correspond both in form and size with that of the boat itself, with the allowance of sufficient room only for the boat to rise and sink freely within it; and its depth and height should be such that the water within it may rise with a loaded boat floating therein from the level of the lower to that of the higher canal: there must also be two sluices one at each end of the lock, large enough to admit the free ingress and egress of the boat, one of which must be accommodated to the level of the higher canal, the other to that of the lower.

Reservoir on one  
side of the lock,

On one side of the lock there must be a reservoir of equal height with that of the lock, the form of which may be rectangular or not (though the former may perhaps be the more convenient) the area of which should be equal to, or rather in practice somewhat exceeding, that of the lock. In this case the perpendicular depth of the reservoir should be equal to double the intended rise or fall of the vessel within the lock, but where the areas of the lock and reservoir are *unequal* this proportion varies: for as in all cases the quantity of water to

be displaced out of the reservoir into the lock must be equal in cubical content to that of the space within the lock between the surface of the lower line and that of the upper, it follows that if the area of the reservoir be greater than that of the lock the necessary depth of the water to be displaced will be so much the less than in the case before stated; and *e contra* if the area of the reservoir be less than that of the lock the necessary depth of the water to be displaced must be so much the greater: so that whether this proportional quantity of water is to be obtained by the greater depth or the greater area of the reservoir, is a point for the consideration of the artist.

There must be a communication somewhere near the bottom and communicating with it, between the lock and reservoir, that the water in each may be always on a level; which level may by means of the lower sluice always correspond with that of the water in the lower canal, except during the actual operation of raising or sinking a boat within the lock.

To the inside of the reservoir must be fitted a \* solid body in which a plunger, in specific gravity somewhat exceeding water, raised or depressed, and of such a bulk and form as will exactly fill the whole of the reservoir, allowing only sufficient room for the plunger to move freely up and down therein.

Let us now suppose the reservoir to be 16 feet deep, and by means of the communication with the lower canal to be precisely *half* full of water. Let us also suppose the plunger to be so suspended by machinery as to be movable up and down within the reservoir, and to be barely above the surface of the water, when it, [the plunger] is at its greatest height. And now let us suppose that a boat or barge is floated through the lower sluice into the lock. That sluice being now shut, as well as the upper one (which was all along supposed to be so) the plunger is let down to the bottom of the reservoir: by this operation though the plunger be not in actual contact with the sides of the reservoir, much less what is called *water-tight*, (a necessary circumstance in the case of forcing-pumps, pistons, &c.) the water in the reservoir will be forced into the lock, and thereby raise the boat or barge as much as the plunger sinks, viz. 8 feet: in which situation the water in the

Method of operation: as the plunger subsides the boat rises and the contrary.

\* At least so far solid as to outweigh its bulk in water.

lock



lock being now on a level with that in the upper canal, the upper sluice may be raised, and the boat floated out. The plunger being still *down*, if a boat be floated into the lock from the upper canal, let the upper sluice be shut and the plunger be raised, and the boat will in like manner descend in the lock to a level with the lower canal.

Hydrostatic principle.

It is obvious that the *principle* of this invention is founded on this law in hydrostatics, viz. that two columns of water however different in lateral dimensions, will, if there be a communication between them, always maintain one and the same level.

The plunger is counterpoised.

The plunger is to be counterpoised by a weight acting on a spiral curve (on the same shaft with the wheels or mechanism that raise the plunger) in order to accommodate the action of the counterpoise to the *decreasing* weight of the plunger as it descends into the reservoir; so that being *counterpoised* it may be easily raised or sunk by means of the mechanical apparatus usually employed for such purposes, possibly indeed this movement may also be effected by means of a fire-engine, but the *expediency* of such means is not meant to be suggested.

General remarks.

In theory the wall or partition common to both the lock and reservoir may not be necessary, however useful it may be in practice.

Should occasion require it, the reservoir may be placed at any given distance from the lock, provided there be a communication between them, at or near the bottom.

Without suggesting any precise limitation (which can be learned only from experience) the projector conceives that locks on this construction are better adapted to low or moderate lifts than to high ones; boats also of a moderate size (as recommended by Dr. Anderson and Mr. Fulton) and so constructed as to exceed rather in *depth* than in length or breadth, seem best adapted to the scheme now proposed.

Advantages of this lock.

On a comparison then between this lock and others of a late invention, it is presumed that wherever it may be expedient to use it, it will be attended with the following material advantages.

No machinery except to the plunger;

1st. The only machinery necessary (the two sluices excepted) in this lock, is for the purpose of raising and sinking the plunger or plungers.

2d. Whilst

2d. Whilst the machinery in other contrivances above al- not liable to  
luded to, is either wholly or in part liable to injury from water, injury from the  
the machinery in this (the two sluices as before excepted) is water;  
wholly exempt from this disadvantage.

3d. Whilst caissons, coffers, &c. require not only the open- nor requiring  
ing of one or more sluices, but also a previous fitting or ad- close fittings.  
justing of their ends to the mouth of the canal to prevent the  
loss of water, before the boat can enter the lock; according  
to this plan, on the opening of a single sluice the boat floats  
at once into the Lock without loss of water, and without any No loss of water.  
such delay or difficulty: when I say without loss of water, it  
must be understood with this allowance, that if a boat goes  
down laden, and returns (or another goes up) unladen, the  
upper level gains a body of water equal to the weight of the  
cargo, *e contra* the lower canal gains similarly if a boat goes  
down unladen and returns laden.

4th. If the water in the upper canal at any time be ever so If water be  
abundant, no boat can pass through the former locks but by abundant, the  
*working the machinery*; whereas according to this plan the machinery need  
*machinery may be at rest*, and the boats pass on as through the not be used,  
common gate locks, whenever the upper canal can afford the  
necessary expence of water.

Lastly. If at any time the machinery of any *one* lock of and therefore  
the former kind (out of the many that may be necessary in a the navigation  
given length of canal) should happen to be disordered, the will not be im-  
*whole navigation is at a stand*, whereas in a lock of this con- peded by any  
struction in case of such an accident, the boats may still pass inevitable dis-  
and repass as through a common gate lock. order.

Let us suppose a boat (or barge) to be 20 feet long, and six Particular state-  
feet wide, and to draw three feet water. The solid content ment of dimen-  
will be 360 cubic feet, each of which weighing about 62 sions, &c.  
pounds, the burthen will be nearly 10 tons.

Is it proposed to raise (or sink) this boat eight feet, by means  
of a plunger of equal area or horizontal superficies with that  
of the boat.

The descent of the plunger must in *this* case be equal to the  
ascent of the boat, viz. eight feet, and the height of the  
plunger must (as in *all* cases) be equal to the sum of both, viz.  
16 feet; the solid content of the plunger therefore will be  
1,920 cubic feet, amounting in weight to nearly 53 tons.

Let

Mechanic force  
saved by enlarg-  
ing the horizon-  
tal section of  
the plunger.

Let us now suppose that *two* such plungers are joined together laterally, so as to form a new plunger, of the same height, but of twice the area and weight of the former. It is plain that this new plunger (moving in a reservoir adapted to its dimensions) will by descending four feet, raise the boat in the lock as much as the other by descending eight feet, viz. eight feet:—and the momenta (consisting of weight multiplied by velocity) of both these plungers will be equal, because the product of 53 [tons] multiplied by eight [the velocity] equals the product of 106 [tons] multiplied by four [the velocity] each amounting to 424.

But it will be found that, as in *all* cases the height of the plunger is equal to the sum of its own descent added to that of the boat's ascent, the height of this new plunger, instead of amounting to 16 feet, will only amount to 12: its weight therefore will be only  $79\frac{1}{2}$  tons, and its momentum 318: it will therefore require so much the less time or labour to work it, in the ratio of 318 to 424, or three to four. Qu. Would not this objection arising from the necessity of having stronger machinery to support this latter plunger, be counterbalanced by the advantage of working it with so great a saving of labour and time?

Remedy for  
deposition of  
sediment.

Should it be apprehended that, notwithstanding the *forcing* effect of the plunger, the water in the reservoir may gradually deposit such a quantity of sediment at the bottom as to obstruct the descent of the plunger, it is obvious, that there may be a depending drain under the reservoir to be opened occasionally, or the bottom of the reservoir itself may be so much below the lowest point of the plunger's descent, as to render the cleansing it very seldom necessary \*.

The machinery  
may be worked  
by waste water.

It remains to be observed, that as every canal ought to be so well provided with water as to allow of *some* waste (though not enough perhaps to supply the loss of a lock-fall with every boat that passes) the machinery of the lock may be worked thereby, without the mechanical aid commonly employed for such purposes; for if the plunger preponderates over the resisting medium, and counterpoise barely enough to overcome

\* The impetus with which the water could rush out of the reservoir into the lock, by the action of the plunger, could probably raise and force out any sediment, except stones or gravel.



the friction of the axis, &c. it will at all times descend slowly within the reservoir, and raise the boat, when left at liberty so to do; and if instead of being allowed to descend the whole eight feet required, it be stopt after having descended seven feet, the admission of one foot depth of water from the upper line into the lock, may be sufficient to raise the plunger again with the loss of only  $\frac{1}{8}$ th part of a lock-fall of water.

Plate XII. Fig. 1. Exhibits a perspective view of a model <sup>View of the</sup> of the apparatus above described, having part of the front <sup>apparatus.</sup> taken away in order to shew the interior parts.

- A. Represents the upper sluice of the lock.
- B. The lower sluice.
- C. The plunger.
- D. The snail upon which the weight or chain is wound that carries the counterpoise.

## VIII.

*An easy Method of Churning Butter.* By CIT. JUMILHAC, President of the Society of Agriculture, of the Seine and the Oise\*.

TO increase the powers of man, is one of the attributes of mechanics, but in order that a mechanical contrivance may be truly useful, it is requisite that it should be simple and cheap. That which I now offer to the society, appears to me to unite these two advantages.

It is universally known, that in great heats and extremely cold weather it is difficult to churn butter. The labour of several persons successively, is often applied without any success, to procure the perfect coagulation of the cream. In vain during the rigour of winter, the cream is placed by the fire, or mixed with milk quite fresh and yet warm; and it is with as little success, that in summer the churn staff is occasionally immersed in cold water. All these means though of value, are insufficient if not seconded by celerity of motion in the act of churning.

Surprised at the slowness with which butter is formed, especially in winter, and after having continued the operation for

\* Sonini's Bibliotheque Phys. Oeconomique, No. 1.

five hours and a half, I thought I saw that the difficulty arose from the awkwardness and constraint of the arm that holds the churn staff; both which are increased in proportion as the cream becomes thicker, as it is then necessary to make a greater exertion to raise and depress the churn staff. This observation is so true, that a person who has churned butter for half an hour at the beginning, without experiencing any fatigue, cannot perform the same work for the space of ten minutes after the cream has become thick and offers more resistance.

Convinced that every interruption, or even diminution of speed, are highly injurious to the butyraceous coagulation, I thought that, without altering either the churn or churn staff, I could adopt a method which is already used in several other operations. Some members of the society think they have already seen this application to the churn staff, but not finding it mentioned in any book, I have presumed, that the society will not object to make it sufficiently public, in order that it may be adopted in all dairies.

Nothing more is required than to fix to one of the beams of the ceiling, a pole three yards long, supported at the distance of two feet by a cross piece, and itself fastened at the end by two blocks or cleats nailed to the timbers. This pole may be about three inches or more in diameter at the upper part, and half that thickness at the lower or moveable end. At the end of the pole is tied a strong cord, which is passed through a hole made near the top of the churn staff, which is so fastened and placed, that it continues suspended in the middle of the churn.

When the churn staff thus fitted up is pressed downward, it enters easily into the cream; but the pole acting as a lever by its elasticity, raises it immediately; this eases the arms, for the moment of the greatest exertion is when you are obliged to raise them as high as the forehead to disengage the churn staff, and immediately force it down again.

Even if this method contributed only to diminish the principal part of the awkwardness and constraint, it would be a great advantage: but I should think this but trifling and incomplete if the contraction of the arm were not remedied. After having considered what powers are jointly employed to produce the effect, I concluded that the position of the arm ought not to be neglected during this long operation; and  
seeking

seeking by trials for the least painful, I determined in favour of that in which the fore arm is employed in the whole motion.

I therefore perforated the handle of the churn staff about twelve inches below the hole through which the cord passes that suspends it to the pole, and I there fastened two small handles of wood of about two centimetres each, which are fastened together by an iron pin; so that the handle of the churn staff resembles a cross. The person who churns places his hands on the two handles, and by a simple pressure, which is so light, that two fingers are sufficient to force the churn staff to the bottom of the churn, he may churn for two hours without experiencing any real fatigue. For the motion of the elevation which alone demands the greatest exertion, is performed by the lever of the pole.

I made use of alder wood for the pole, as one of the most elastic and least likely to warp.

We see from this description,

1. How little it costs to establish the mechanical help, which only consists in a pole of alder wood of three yards long, and two small wooden handles adapted to the churn staff.

2. How greatly it assists and increases the power of the person who churns, by obviating the most painful movement, by placing the hands naturally, so as to require the motion of the fore arm only, which is evidently the least fatiguing.

I am convinced by the result of constant practice, that one third of the time is saved which is usually employed in making butter.

## IX.

*Description of a Machine for rooting up the Stumps of Trees.*

By CIT. SAINT VICTOR, Member of the Society of Agriculture, for the Department of the Seine\*.

EVERY cultivator is well aware how detrimental the stumps of trees are which remain in the ground, which instead of being rooted up at the time they were cut down, have been sawed off close to the stump, to save the expence of digging them out of the earth. Inconveniencies of leaving the stumps of trees after felling the trunk.

\* Bibliotheque Phys. Oeconomique, de Sonini, No. 1.



Instance.

In an estate in Savoy, I was subjected to these inconveniences, from the avarice of the proprietor who had felled a great number of large trees, and sawed them off close to the roots, these were oaks, walnuts, and chefnuts, which grew in the meadows and ploughed grounds, and he disposed of them as timber in planks and fire wood, without taking any care to have them rooted up at the same time, which would have been then more easy, by the common practice of using the trunk as a lever.

Attempt to blow them up by gunpowder.

When I came into possession of this estate, I foresaw that I could by no means avoid rooting up a great number of stumps that were very injurious to every kind of cultivation; and to avoid the long, troublesome, and expensive labour of digging great trenches, which are usually required to be made, in order to bring up the sort of roots, I thought I might employ the force of gunpowder. This attempt perfectly succeeded, by means of a little machine of iron I had forged, of which I here join a plan, section, and perspective view, which shews the manner of placing it beneath the stump \*.

#### *Description of the Machine.*

Machine for blowing up the roots of trees.

It consists of a bar of forged iron, about two feet eight inches long, one inch thick towards the handle, and of two inches towards the breech or platform.

It is a small mortar with a plug and handle.

The platform which is circular, is 14 inches in diameter.

This platform serves as the base of the chamber, or furnace of the mine, which is three inches in diameter, and three inches eight lines in the length of its bore.

\* The use of this ingenious machine ought not to be confined merely to the stumps of trees in our fields. Licentiousness and avarice within the last five years, have deprived our country of a quantity of woods and thickets, and degraded by immense fellings, our noblest forests, the incalculable source of riches for the present, and hope for future time. Whether these spaces thus impoverished be converted into arable ground or pasture, or whether the imperious necessity or claims of posterity should require that they should again be planted with trees, the invention of Cit. Saint Victor, will be a most useful application to eradicate from the ground those roots which would deprive us of a part of its product.

*Note of the editor.*

The

The stopper or tampion which serves as a plug to the mine, is of the same diameter, to enter within after a slight paper or wadding. It is attached by a chain to the gun or mortar, which last is eight inches in diameter.

About two inches above is added a small touch hole and pan. The hole is directed in an angle of 45 degrees, and is primed with powder to communicate with the charge with which the chamber is filled up to the stopper.

This engine may be cast even with more facility in brass or bronze, and in this case, it must be a little thicker in all its dimensions, in order to afford a resistance equal to that of the forged iron.

#### *Use of the Machine.*

When the machine is charged with powder, a small excavation is made with a pick axe, in the center of the stump. The machine is then placed in it, so that the plug immediately touches the wood. Care must be taken to fill all the vacancies, either with stones, or pieces of iron, or wood, more especially beneath the platform of the machine, in order that the explosion of the powder may have its full effect on the stump, of which if necessary, the principal roots should first be cut, if any appear on the surface of the ground near the stump, that is to be eradicated.

When the machine is firmly fixed in its place, the priming is put into the pan, a slow match applied, the length of which is sufficient to allow time to retire to a proper distance from the explosion.

By inspection of the plan and section, every one will perceive the utility of this simple machine, and it may easily be made by any intelligent smith.

A. Plate XIII. Fig. 1. Plan of the machine of about two feet three inches long. Delineation and plate.

B. Plan of the machine, 10 inches high, comprehending the plug.

a. The plug with its cap fastened to the chain.

b. The chamber for the powder.

C. The touch hole.

The middle figure represents the machine placed under the stump of a tree \*.

\* Mr. Knight, ironmonger, of Foster Lane, has contrived a simple apparatus for splitting blocks of wood with gunpowder, of which the description will be given in our next.—N.

*Method*

## X.

*Method of Secret Writing, by means of a Steganographic Scale.*

*By J. B. BERARD \*.*

Methods of secret writing.

AN infinite variety of methods of steganography or indecipherable writing has been devised; of these, some are defective in theory, and others inconvenient in practice. It is not my intention to discuss the advantages or inconveniences of all those which are known, it would lead me too far. I shall therefore confine myself to noticing a few of the principal ones.

Alphabets of convention.

Alphabets of convention afford but little security; the characters which perform the functions of vowels and consonants, recur in such situations, as to make their use very apparent; and it is ascertained, that by patience and a little skill the secret is easily discovered.

Sympathetic ink.

The sympathetic inks, seven in number, are not more safe, because after several trials, that re-agent which renders the writing legible, is at length discovered.

Folding the paper.

Those methods which depend on folding the paper in a particular manner, as was practised by the Spartans, are inconvenient, and afford but little defence against curiosity.

Kircher's method.

The method of Kircher, though sufficiently certain in its principle, is of little value in practice; besides which, its performance is both tedious and inconvenient: a point wrong placed or omitted, is sufficient to render the secret unintelligible to the correspondent.

General enumeration.

In the sixth number of the *Journal de l'Ecole Polytechnique*, p. 382, is a table, in which Cit. Hassenfratz has classed all the modes of correspondence, whether by writing or otherwise. That which I have devised, is as follows; it will perhaps be found to possess the double merit of simplicity and security †.

Take

\* *Melanges Physico-Mathematique.*

† This method has been made use of for the private correspondence of Cit. Forfait, minister of the marine, and the colonies, and president of the lyceum of arts. There is no inconvenience in mentioning the fact; for the advantage of a good method consists in



Take a rule of pasteboard, wood, ivory, or copper, about one inch broad and seven inches long, divide its edge into 30 equal parts, and between the divisions write the first thirty natural numbers, from 1 to 30 inclusive, in any arbitrary order. The following is the manner of using this rule.

*Operation for writing.* First make a minute or outline of the secret you wish to send, then place the rule or steganographic scale on the paper intended for the secret, and mark the two ends of the rule by two small lines of ink, the reason of which will be seen below.

This being done, transcribe the first thirty letters of the minute, writing the first letter opposite to the figure 1, on the rule, the second opposite to the figure 2, the third opposite to the figure 3, and so on to the end.

Bring down the rule and transcribe in a similar manner, opposite to the figures, the next thirty letters of the secret, that is to say, from the thirty-first to the sixtieth inclusive.

Continue thus transcribing in new lines, each consisting of thirty letters, until the whole secret is written.

The punctuation must be carefully inserted to the right of that letter which is on its left in the minute, and a mark like this + must be placed above the last letter of each word, to distinguish it from that which follows, and thus render the reading easy to the correspondent.

*Operation of the correspondent.* The correspondent who receives the secret with the letters thus misplaced, will be able to transcribe it, and replace the letters in their proper order, by inverting the preceding operation.

For this purpose, he must be provided with a rule similar to that made use of for deranging the letters of the secret. He will place it on the first line, so that the letters correspond with the figures on the rule. This will be easily accomplished by means of the two lines of ink which mark the ends of the rule.

It will then be easy for him to transcribe the letters of the secret in their proper order, beginning with that opposite to

in its capability of being employed by all the world, without diminishing its secrecy for each individual; it will be seen that, in this plan, it is the secret of the series agreed on, and not that of the method, which renders the correspondence safe.

figure

New method by a scale of transposition.

The process. To write.

To decypher.



with four series, and may serve for four correspondents: if it be of ivory, the series may be changed at pleasure, by writing the figures with a pencil: the scale may even be composed of thirty small detached moveable pieces of brass slipped upon a flat iron rod, and secured with a nut at one of its ends.

3. It may be easily conceived, that the principles of permutation may be varied to infinity, for instance, by changing only the order of the syllables, the words, or the lines. The above method on which I have fixed, appears to be at once easy, certain, and concise, and to require the simplest apparatus. Other permutations.

4. If it be desired to conceal even the appearance of a secret, the method above described may be employed to write with one of the sympathetic inks, at the end of an ostensible subject.

5. *Another method.* Having divided the paper into 100 or 200 squares, write in each a letter, a syllable, or a word of the secret, and also one of the numbers of the concerted series: separate and mix the squares; the correspondent will only be required to replace them in their order by means of the figures of the series. Another method by the scale,

For the greater security, the edges of the squares should be pared to destroy the connection of the adjoining sides.

6. *Method of intercalations.* Draw on pasteboard, some parallel divisions about half an inch asunder, pierce them with holes, either equal or unequal, of an arbitrary size, and at any distance from each other: lay the pasteboard on the paper, and write the secret by inserting successively from left to right, a letter, syllable, or word in each vacancy: remove the pasteboard and fill up the intervals with insignificant letters; or it will be better, if time permit, that the letters should form with those of the secret, a reasonable meaning, not liable to be suspected. another by intercalation.

The correspondent will easily read the secret through holes in a similar pasteboard.

The insertion of unmeaning letters may be avoided by using three or four pasteboards of equal size, but so pierced, that when they are laid on each other, all the holes will be closed. By writing through each of these in succession, all the lines will be filled. The correspondent must be provided with similar pasteboards, and will read the secret through them by applying them successively to the writing.

This



This method is shorter than that of the anagrammatic scale, and is preferable when there is much to write; but it is less secure, and requires a more complicated apparatus.

# XI.

*Description of a Magazine Pistol, which when loaded is capable of being discharged Nine successive Times through the same Barrel.*  
W. N.

Account of a  
magazine pistol.

I AM indebted to the liberality of the Right Honourable Lord Camelford for the accurate drawings of the curious and valuable piece which forms the subject of the present Memoir, as well as for permission to use and examine it in any manner I might think proper. It is of German make; the workmanship very good, and it has been used by his Lordship without any particular care in various parts of the world. Its construction does, in effect, shew that its use is attended with neither danger nor uncertainty; but I shall postpone my remarks till I have given the description.

Description by  
reference to the  
drawings.

Figs. 1 and 4 in the Plates XIV. and XV. exhibit views of the two sides of the pistol. Fig. 5. is a section to shew the magazines, and Figs. 2. and 3. shew what may be called the chamber piece. The large face of this last (Fig. 2.) is slightly taper or conical from X towards Y, where the diameter is smallest, and the small part B is cylindrical, excepting that a portion is scooped out on one side marked by the dark space D in Fig. 3. Its proper place in the pistol is in the breech, crossing the line of direction at right angles. When the lock is off, it goes into its cell from the side Fig. 1. which it fits with considerable accuracy, but does not come to rub hard, because the Plate X forms a projecting face which stops it. In this situation its small cylindrical part B projects outwards, and is received in an hole of the same diameter in that part of the lock beneath the hammer where the pan is usually placed. In fact, the cavity D, Fig. 3. does constitute the pan when its position is such as to be immediately beneath the covering face of the hammer. The screw B which goes into the cylindrical piece, and Z, which goes into the stock, are the fastenings by which the lock is secured. At the opposite end

A,

A, Fig. 2. the chamber piece terminates in a square, upon which the broad head of the lever L, Fig. 4. is fitted, and kept down by the screw A, which goes into the chamber piece. It is to be understood that the cock and hammer are constructed, and act in the same manner as in the best fire arms, and do not therefore require to be described. The lever is capable of being moved from the position M to those of L and N; but is prevented from describing the remaining part of the circle by an interior stop, which may be easily imagined without any attempt at minute explanation.

Fig. 5. being a section through the middle of the stock, breech, and part of the barrel, shews the relative situation of the magazines, with the chamber piece, and other parts. The balls S, 2, 3, 4, &c. are lodged in their proper receptacle, being put in through the hole R, Fig. 4. and the powder is lodged in its magazine Q Q. both which are closed by the door T, which is then fastened by a small bolt and back spring. A, Fig. 5. shews the situation of the chamber piece at the time when the lever is brought to the position M. This is done with the muzzle of the piece pointing towards the ground, and the effect is, that powder runs into the chamber P (see also Figs. 2 and 3) and a ball into the chamber S. The lever is then moved from M to L and N, by which process the ball chamber drops its ball into the barrel as it passes opposite to N, where the ball remains, because the actual bore of the screw barrel is not wide enough to allow it to pass farther than just to clear the moveable chamber. As soon as the lever has arrived at the position N, the powder chamber P is exactly opposite the ball, and ready to be discharged against it. After the discharge the muzzle is to be again depressed, and the lever moved from N to M: the chambers become again charged with powder and ball; and by returning the lever back to N, this ball and powder become duly placed for a second discharge. It is obvious that these discharges may be repeated until all the balls have been fired out. The small bridge in the powder chamber P (Fig. 2.) is to prevent any impediment from the entrance of part of one of the balls into the receptacle, and the perforations W, W, in the breech, serve to clear the surface of the chamber piece from any foulness it might acquire.

Thus

Description of a  
pistol which fires  
nine balls in  
succession by  
once charging.

Thus far we have spoken only of loading and discharging ; but this piece would admit of little rapidity of effect, if it did not at the same time cock and prime itself. It may be observed, that the projecting part, or stud C, is fixed to the chamber piece Fig. 2. and 3. a very little behind the shot chamber ; so that it stands at the top of the lock at the time of charging. A thin flat bar of iron proceeds from the cock, and slides with it along the face of the lock plate, within the hammer : against this plate the stud C acts, and brings it to full cock a little before the lever arrives at the position M ; at the same time that the more prominent part of C presses on the back of the hammer, and shuts the pan. The lock is therefore put into the condition to give fire by the same simple operation, and precisely at the same moment as the charge is taken from the magazines.

It now remains only to be shewn how the priming is given. The excavation D in the side of the small cylinder B, Fig. 3. constitutes the pan, and the small dot represents the touch hole passing through the metal into P the powder chamber. When the lever is in the position M, Fig. 4, the cavity D is exactly placed beneath the covering face of the hammer G : but at the time of charging it has the position Fig. 3. considered with regard to that of Fig. 1. In this last figure the dark shaded space H denotes a reservoir for priming, the door of which may be opened and shut under the action of an ingenious back spring, operating nearly like that of a clasp knife. A long perforation or slit communicates from this reservoir to the space in which the cylinder B revolves : so that when the excavation D passes that slit (during the return of the lever) it becomes filled with powder, which it carries round to the last position, which is exactly that in which it must receive the fire whenever the pistol is to be discharged.

The history of this construction seems to be imperfect ; but there is reason to suppose that it is of some antiquity. The magazine air gun of Colbe constructed for carrying ten balls, and lodging them successively in the barrel by a cross cylindrical piece was made early in the last century, and is described in Defagulier's Lectures, and most elementary books. In the present arm the contrivances are highly judicious, as well with regard to mechanism as to arrangement. If it were possible for the powder magazine to be set on fire at the distance

tance



tance of a semi-circumference of the chamber piece from the explosion, the only effect would be that the door would be blown open, and this is situated in a place where it could do no harm. The same remark is also applicable to the magazine for priming.

Description of a pistol which fires nine balls in succession by once charging.

How great the advantages must be in battle, for a man to be able to reload his piece by a simple movement of one second of time, without taking his eye off his enemy; or how considerably useful this invention might prove in the defence against robbers need not be stated. It can indeed be stated, that the opponent may also provide himself with the like advantage; and then we have only to urge the argument, that the duration of wars have been diminished, and its humanity diminished by rendering the means of annoyance more perfect.

I have discharged this course of balls several times, and I find that the whole nine balls can be fired in 30 seconds.

## XII.

*On the Diffemination of Plants.* By CIT. L. REYNIER \*.

THE history of vegetables affords some facts to which I think it my duty to call the attention of naturalists: they relate to the diffemination of plants, and to the means by which this is effected. I have already collected several observations on this subject in the dictionary of Agriculture of *L'Encyclopédie Méthodique*, article *Diffemination*.

There are two natural means of reproduction: one of these is by the roots, which, spreading outwards, form new stems round the mother plant; this reproduction is slow, and can only take place gradually and without intervals; the other is by the seeds, which being carried by the winds, or by the hooks with which they are provided, or by animals which swallow them, and afterwards deposit them, unchanged, in their excrements, are removed to greater distances, though still within a limited circle. It is not therefore wonderful, to see a plant spring up in a spot, where the same species is known to exist at no great distance; its introduction is in the class of possibilities.

\* Decade Philosophique, No. 13. An. XI.

But

New vegetations that appear when the face of the ground is disturbed.

But that which ought to excite our attention, is the production of a new vegetation in a place which has undergone some very great changes. In all cases when the surface of a place has been altered either by the falling or removal of the earth, by opening roads through forests, or by draining marshes, plants are sure to be found in the following year, which did not exist there before, while the former species disappear, except a few cosmopolites, if the expression may be allowed, which, though they re-appear, undergo a remarkable alteration (*Dict. d'Agr. de L'Encycl. meth. art. Climat.*) When a wood is grubbed up, the forest plants cease to grow, and those natural to tilled ground immediately germinate. Labat notices (see Vol. I. p. 386.) that, in his time, at the Antilles, as soon as the ground was cleared, its surface became covered with purslane. In those places where charcoal is made, in forests at a great distance from any habitation, I daily observe plants, different from those which were formerly there, and which are natives either of dry pastures or cultivated lands, such as the (vergerette) of Canada, the annual veronicas, &c. Forster, in his voyages with Cook, took notice of several islets in the middle of the sea, simply rocks of coral, on which the first rudiments of vegetation were forming. In the low valleys of the downs of Holland, I have gathered a satyrium and an ophrys, natives of the summits of the Alps and the plains of Spitzbergen (*Satyrium viride*, *L. Ophrys monorchis*, *L.*) But these downs, according to the observations of Cit. Décan-dolle, (*Ann. d'Agr. T. XIII. p. 372.*) are not of very ancient origin. How is the introduction of these two plants, which are naturally provided with very limited means of multiplication, to be accounted for? I have expressed a wish in the Encyclopedie, that the new islands formed by volcanos, and particularly those near Santorini should be examined. One of our most accurate observers is on the point of visiting the latter, (*See d'Olivier, T. II. p. 238.*) The new one produced by the eruption of 1707 to 1711, does not yet shew any appearance of vegetation, the air is still mephitic; that which arose in 1573 has some traces of vegetation, particularly some grasses, and a small fig tree; the latter may have been deposited by some bird, but by what means were the grasses conveyed?

Are these produced by the roots or seeds of other plants.

It is true that seeds which are buried to a certain depth will be preserved for a great length of time, and being afterwards submitted by culture to the action of the light and water, will germinate. But is it to be presumed, that the seeds of purslane were preserved in the earth of the Antilles, during the many ages the forests had endured before they were destroyed? Is it probable that, in the forests of Gaul, whose antiquity is well ascertained, the seeds of those plants, which appeared after they were rooted up, or after placing a charcoal furnace in them, should have been preserved sound from before the existence of the forests? The dissemination of plants has necessary limits; beyond these, we must have recourse to other explanations.

Could the seeds have been so long preserved.

Lastly, the plants of the class cryptogamia, and particularly fungi, whose form is constantly determined by the nature of the substance on which they grow, and is constantly the same in similar circumstances, although they do not appear at stated seasons like other vegetables, also present new facts. It is not long since Cit. Ventenat observed a boleta of a peculiar new shape, which was produced on a human body. I have noticed the clavaria, which is constantly formed on the crysalides of caterpillars (*Journal de Phys. Année. 1787*); others grow on those fruits which have a woody covering; such as are formed on the fragments of fir, are not the same as those which grow on the wood of the oak. The husk of the nutmeg produces a fungus peculiar to itself, (*Ancienne Encycl. art. Muscade*). Lastly, the fungi which are formed on the wooden props of mines, cellars, &c. are not the same as those produced by the same woods when they decay in the open air. It is difficult to conceive the dissemination of seeds from one mine to another, particularly to those which are opened in situations where no mines had existed before; nevertheless I have found all those which I have examined in different countries to possess very nearly the same form.

Facts respecting the cryptogamia.

All these considerations may induce us to presume, that nature is daily exercising the same powers which she possessed at the creation, and it would be interesting to examine the causes and ascertain the means of execution; but this can only be the consequence of long and diversified investigations, carried on by a great number of observers.

It is presumed that plants continue to be formed without propagation.



## XIII.

*An Essay on the Declivities of Mountains. By RICHARD KIRWAN, Esq. L. L. D. F. R. S. and P. R. I. A,*

Introductory remarks.

AMONG the various causes to whose activity the planet we inhabit owes its present wonderfully diversified appearance, some undoubtedly exerted their influence from its very origin, and others at subsequent periods; of these last one at least, namely, the Noachian deluge, was universal in its operation, while the effects of many more were partial and local, such as those resulting from earthquakes, volcanos, particular inundations, &c.

In a general survey of the globe, it is only to general causes, whose operation was universal, that our attention can be directed; the effects of partial causes being the proper objects of the geological history of those countries that were particularly affected by them.

But to distinguish causes of the former class from those whose operation was more confined, it is necessary to discover some character by which their effects may unequivocally be discerned.

Now a general *uniformity*, or agreement in some particular circumstance in every part of the globe, seems to be a sure test of the operation of some general cause. The discovery of uniform appearances is therefore of primary importance in geological researches. In the present essay I shall confine myself to the investigation of one instance of this sort, namely, the *inequality of declivity* which the sides or flanks of mountains exhibit in every part of the globe hitherto examined according to the points of the compass to which they face, and are exposed.

All high mountains or hills have one side much steeper than the other.

That one part of almost every high mountain or hill is steeper than another, could not have escaped the notice of any person who had traversed such mountains; but that Nature in the formation of such declivities had any regard to different aspects or points of the compass, seems to have been first remarked by the celebrated Swedish geologist Mr. Tilas, in the 22d vol. of the

Memoirs

Memoirs of Stockholm for 1760\*. Neither Varenus, Lulolph nor Buffon in his Natural History published in 1748, have noticed this remarkable circumstance.

The observation of Tilas however relates only to the extreme ends, and not to the flanks of mountains; with respect to the former, he remarked that the *steepest* declivity always faces that part of the country where the land lies lowest, and the *gentlest* that part of the country where the land lies highest, and that in the southern and eastern parts of Sweden they consequently face the E. and S. E. and in the northern the W. The essential part of this observation extends therefore only to the general elevation or depression of the country, and not to the bearings of these declivities.

The discovery that the different declivities of the flanks of mountains bear an invariable relation to their different aspects, seems to have been first published by Mr. Bergman in his physical description of the earth, of which the second edition appeared in 1773. He there remarked that in mountains that extend from N. to S. the western flank is the *steepest*, and the eastern the *gentlest*. And that in mountains which run E. and W. the southern declivity is the *steepest* and the northern the *gentlest*, vol. 2d. § 187.

This assertion he grounds on the observations related in his first vol. § 32, namely, that 1<sup>o</sup> in Scandinavia the Suevoberg mountains that run N. and S. separating Sweden from Norway, the western or Norwegian sides are the *steepest*, and the eastern or Swedish the most moderate, the verticality or steepness of the former being to that of the latter as 40 or 50 to 4 or 2 †.

2dly. That the Alps are steeper on their western and southern sides than on the eastern and northern.

3dly. That in America the Cordelieres are steeper on the western side, which faces the Pacific Ocean, than on the eastern. But he does not notice a few exceptions to this rule in particular cases which will hereafter be mentioned.

Buffon, in the first vol. of his Epochs of Nature published in 1778, p. 185, is the next who notices the general prevalence

The steep side faces the low country. Tilas.

The western side is steepest. Bergmann.

Instances. In Scandinavia.

The Alps.

The Cordelieres.

Remarks of Buffon,

\* See also vol. 25, Swed. Abhandl. p. 291, where Cronsted explains some obscure parts of Tilas's observation.

† The verticality of the sides is inversely as the length of the descent.

of this phænomenon, as far as relates to the eastern and western sides of the mountains that extend from north to south, but he is silent with respect to the north and south sides of the mountains that run from east to west; nay, he does not seem to have had a just comprehension of this phenomenon, for he considers it conjointly with the general dip of the regions in which these mountains exist. Thus he tells us, vol. 1st, p. 185, that in all continents the general declivity, taking it from the summit of mountains, is always more rapid on the western than on the eastern side; thus the summit of the chain of the Cordelieres is much nearer to the western shores than to the eastern; the chain which divides the whole length of Africa, from the Cape of Good Hope to the mountains of the Moon, is nearer, he says, to the western than to the eastern seas; of this however he must have been ignorant, as that tract of country is still unknown.

The mountains which run from Cape Comorin through the peninsula of India are, he says, much nearer to the sea on the east than on the west; he probably meant the contrary, as the fact is evidently so, and so he states it in the 2d vol. p. 295; the same he tells us may be observed in islands and peninsulas, and in mountains.

and other authors;

This remarkable circumstance of mountains was notwithstanding so little noticed, that in 1792 the author of an excellent account of the territory of Carlsbad in Bohemia, tells us he had made an observation, which he had never met with in any physical description of the earth, namely, that the southern declivity of all mountains was much steeper than the northern, which he proves by instancing the *Erzgebirge* of Saxony, the Pyrenees, the mountains of Switzerland, Savoy, Carinthia, Tyrole, Moravia, the Carpathian and Mount Hæmus in Turkey, 2, *Bergm. Jour.* 1792, p. 385, in the note.

Herman;

Herman in his *Geology*, published in 1787, p. 90, has at least partially mentioned this circumstance, for he says that the eastern declivities of all mountains are much gentler and more thickly covered with secondary strata, and to a greater height, than the western flanks, which he instances in the Swedish and Norwegian mountains, the Alps, the Caucasian, the Appenine and Ouralian mountains; but the declivities bearing a southern or northern aspect he does not mention.

La Metherie;

La Metherie, in the 4th vol. of his *Theory of the Earth*, of which the second edition appeared in 1797, a work which



abounds in excellent observations, p. 381 \*, produces numerous instances of the inequality of the eastern and western declivities, but scarce any of the northern and southern, whose difference he does not seem to have noticed, but he makes a remark which I have not seen elsewhere, that the coasts of different countries present similar declivities.

With regard to eastern and western aspects, he thinks that a different law has obtained in Africa from that which has been observed in other countries, for in that vast peninsula he imagines the eastern declivities of mountains are the steepest, and the western the gentlest. Of this however he adduces no other proof, but that the greatest rivers are found on the western side: this proof seems insufficient, as, if mountains be situated far inland, great rivers may flow indiscriminately from any side of them, and sometimes few rivers flow even from the side whose descent is most moderate, for instance, from the eastern side of the mountains of Syria; the Elbe and the Oder, two of the greatest rivers in Germany, take their course from the western sides, the first of the Bohemian and the other of the Moravian mountains, which yet are the steepest. Many originate from lakes, as the Shannon with us; many take such a winding course, that from a bare knowledge of the place of their disembogement it is impossible to judge from what side of a mountain they issue, if from any; their course at most discovers the depression of the general level of the country.

In 1798, the celebrated traveller and circumnavigator, John J. R. Foster; Reinhold Foster, published a geological tract which merits so much more attention, as all the facts were either observed by himself, or related to him by the immediate observers. In this he states as a fact universally observed, that the south and south-east sides of almost every mountain are steep, but that the north and north-west sides are gently covered and connected with secondary strata in which organic remains abound, which he illustrates by various instances, some of which have been already, and others will presently be mentioned.

Universal fact: that the S. and S. E. sides of mountains are steepest.

At present this fact attracts the greatest attention, being obviously connected with the original structure of the globe, and clearly proving that mountains are not mere fortuitous eruptions unconnected with transactions on the surface of the earth, as has of late been confidently advanced.

\* It is to be regretted that he scarce ever quotes his authorities.

I shall now state the principal observations relative to this object, that have been made in different parts of the world.

*In Europe.*

Account of  
mountains: in  
Europe,

1. The mountains that separate Sweden from Norway extend from north to south, their western sides are *steep* and the eastern *gentle*, 1. Bergm. Erde Beschreib. p. 157.

2. The Carpathian mountains run from E. to W. their southern sides towards Hungary are *steep*, their northern towards Poland moderate, Foster, § 46.

3. Dr. Walker, professor of natural history at Edinburgh, observed that the coasts and hills of Scotland are steeper and higher on the western side than on the eastern; Jamison's Mineralogy of Shetland, p. 3. However, Jamison observed, that the south side of the isle of Arran is the lowest, and the north side the highest, p. 51.

4. The mountains of Wales are gentle on the eastern and steep on the western sides.

5. The mountains of Parthery, in the county of Mayo, are steep on the western side.

6. The mountains which separate Saxony from Bohemia descend gently on the Saxon or northern side, but are steep on the Bohemian or southern side; Charpent, p. 75. The southern declivity is to the northern as six to two, 2d Bergm. Journ. 1792, p. 384 and 385.

7. The mountains which separate Silesia from Bohemia run nearly from E. to W. yet are steeper on the northern or Silesian side than on the opposite Bohemian; Assemani *Silesia*, 335. Such branches as run from N. E. to S. W. have their western covered with primordial strata, and consequently less steep; 4. New Roz. p. 157.

8. The Meissener in Hesse is steeper on the N. and E. sides which face the Warra, than on the south and western; 1. Bergm. Journ. 1789, p. 272.

9. The mountains of the Hartz and Habichtswald are steep on the south and gentle on the northern sides, Foster, § 46.

10. The Pyrenees, which run from E. to W. are steeper on the southern or Spanish side; Carbonieres, XIII.

11. The mountains of Crim Tartary are gentle on the northern and steep on the southern sides, Foster, *ibid.*

*In Asia.*

12. The Ourals, which stretch from N. to S. are far steeper in Asia, on the western than on the southern sides; Herman *Geologie*, p. 90, and 2d *Ural Beschreibung*. p. 389.

13. The mountain of Armenic to the west of the Ourals is steep on its E. and N. sides, but gentle on the southern and western; 1. *Pallas Voy.* p. 277.

14. The Altaïshan mountains are steep on their southern and western sides, but gentle on the northern and eastern; Foster, *ibid.* and Herman 2d. *Ural Beschreibung*. p. 390, in the note.

15. So also are the mountains of Caucasus, 3d. *Schrift. Berl. Gefelsch.* 471.

16. The mountains of Kamskatska are steep on the eastern sides, Pallas, 1 *As. Petropol.* 1777, p. 43.

17. The Ghauts in the Indian peninsula are steep on the western sides.

18. The mountains of Syria which run from N. to S. skirt-ing the Mediterranean, are said to be steeper on the western side facing the Mediterranean; 4. *La Metherie*, p. 380.

*In America.*

The Cordelieres run from N. to S. their western flanks towards the Pacific are steep, their eastern descend gradually.

In Guiana there is a chain of mountains that run from E. to W. their southern flanks are steep, their northern gentle; *Voyages de Condamine*, p. 140.

To assign the causes of this almost universal allotment of equal declivities to opposite points, and why the greatest are directed to the west and south, and the gentlest on the contrary to the east and north, it is necessary to consider, Deduction of causes.

1. That all mountains were formed while covered with water.

2. That the earth was universally covered with water at two different æras, that of the creation, and that of the Noachian deluge. The earth was originally covered with water.

3. That in the first æra we must distinguish two different periods, that which preceded the appearance of dry land, and that which succeeded the creation of fish, but before the sea had been reduced nearly to its present level; during the former Two æras.



former, the primæval mountains were formed, and during the last, most of the secondary mountains and strata were formed.

4. That all mountains extend either from E. to W. or from N. to S. or in some intermediate direction between these cardinal points, which need not be particularly mentioned here, as the same species of reasoning must be applied to them, as to those to whose aspect they approach most.

In the first the primæval mountains were formed, and the waters moved from E. to W. and from N. to S.

These preliminary circumstances being noticed, we are next to observe that during the first æra, this vast mass of water moved in two general directions, at right angles with each other, the one from E. to W. which needs not to be proved, being the course of tides which still continue, but were in that ocean necessarily stronger and higher than at present: the other from N. to S. the water tending to those vast abysses, then formed in the vicinity of the south pole, as shewn in my former essays. Before either motion could be propagated, a considerable time must have elapsed.

The primæval mountains resisted the waters, which occasioned the greatest depositions on the N. and E. sides.

Now the primæval mountains formed at the commencement of the first æra, and before this double direction of the waters took place, must have opposed a considerable obstacle to the motion of that fluid in the sense that crossed that of the direction of these mountains. Thus the mountains that stretch from N. to S. must have opposed the motion of the waters from E. to W. this opposition diminishing the motion of that fluid disposed it to suffer the earthy particles with which in those early periods it must have been impregnated, to crystallize or be deposited on these eastern flanks, and particularly on those of the highest mountains, for over the lower it could easily pass; these depositions being incessantly repeated at heights gradually diminishing as the level of the waters gradually lowered, must have rendered the eastern declivities or descent, gentle, gradual, and moderate, while the western sides receiving no such accessions from depositions, must have remained steep and craggy.

Again, the primæval mountains that run from E. to W. by opposing a similar resistance to the course of the waters from N. to S. must have occasioned similar depositions on the northern sides of these mountains against which these waters impinged, and thus smoothed them.

Where

Where mountains intersect each other in an oblique direction, the N. E. side of one range being contiguous to the S. W. flanks of another range, there the afflux of adventitious particles on the north-east side of the one, must have frequently extended to the S. W. side of the other, particularly if that afflux were strong and copious; thus the *Erzgebirge* of Saxony, which run from W. to E. have their N. E. sides contiguous to the S. W. side of the *Riesengebirge* that separate Silesia from Bohemia, and hence these latter are covered with the same beds of gneiss, &c. as the northern sides of the Saxon; and thereby are rendered smooth and gentle comparatively to the opposite side, which being sheltered, remains steep and abrupt, which explains the seventh observation.

Mountains near each other would intercept this; deposition.

The causes here assigned explain why the covering of adventitious strata on the highest mountains is generally thinnest at the greatest height, and thickest towards the foot of the mountain, for the bulk of the water that contained the adventitious particles being proportioned to its depth, and the mass of earthy particles with which it was charged, being proportioned to the bulk of water that contained them, it is plain, that as the height of water gradually decreased, the depositions from it on the higher parts of the mountains must have been less copious than on the lower, where they must have been oftener repeated.

It would be least at the greatest heights, &c.

Hence, 2. granitic mountains, generally the most ancient, frequently have their northern or eastern sides covered with strata of gneiss or micaceous schistus, and this often with argillite, or primæval sand-stone, or lime-stone, these being either of somewhat later formation or longer suspendible in water.

Hence, 3. different species of stone are often found at different heights of the same flank of a mountain, according as the water which conveyed these species, happened to be differently impregnated at different heights; during the first æra its depositions formed the primitive stony masses, but after the creation of fish, lime-stone, sand-stone, facillites and secondary argillites, in which piscine remains are found, were deposited. But during the second æra, viz. that of the Noachian deluge, by reason of the violence and irregularity of its aggression, the depositions were more miscellaneous and are found at the greatest heights; yet in general they may well



well be distinguished by the remains of land animals, or of vegetables, or of both, which they present in their strata (or at least by the impressions of vegetables which they bear) as these must have been conveyed after the earth had been inhabited. But mountains regularly stratified bearing such remains, for instance the carboniferous, cannot be deemed to have been formed in a period so tumultuous. During this deluge the waters also held a different course, proceeding at first from south to north, and afterwards in both opposite directions, as shewn in treating of that catastrophe in my second essay.

Exceptions from  
local causes.

Hence, and from various contingent local causes, as partial inundations, earthquakes, volcanos, the erosion of rivers, the elapsion of strata, disintegration, the disruption of the lofty mounds by which many lakes were anciently hemmed in, several changes were produced in particular countries that may at first sight appear, though in reality they are not, exceptions to the operations of the general causes already stated.

Instances.

Thus the mountains of Kamskatska had their eastern flanks torn and rendered abrupt by the irruption of the general deluge, probably accompanied by earthquakes. And thus the Meissener had its E. and N. flanks undermined by the river Warra, as Werner has shewn; thus the eighth and sixteenth observations are accounted for, as is the thirteenth, by the vast inundations so frequent in this country, 1 Pallas, p. 172, which undermined or corroded its E. side, while the western were smoothed by the calcareous depositions from the numerous rivers in its vicinity.

Different species  
of stone must be  
found on differ-  
ent sides of  
mountains.

Hence, 4. we see why on different sides of lofty mountains different species of stones are found, as Pallas and Saussure have observed, 2 Sauss. § 981, a circumstance which Saussure imagined almost inexplicable, but which Dolomieu has since happily explained, by shewing that the current which conveyed the calcareous substances to the northern, eastern, and north-eastern sides of the Alps, for instance, was stopped by the height of these mountains, and thus prevented from conveying them to the southern sides, and thus the north-eastern sides were rendered more gentle than the opposite, 3. New Rotz. p. 425! conformably to the theory here given.

Interceptions.

Hence, 5. where several lofty ridges run parallel to each other, it must frequently happen that the external should intercept



tercept the depositions that do not surmount them; and thus leave the internal ridges steep on both sides.

Hence, 6. low granitic or other primitive hills are frequently uncovered by adventitious strata on all sides, as at Phanet in the county of Donegal, or are covered on all sides; the impregnated waters either easily passing over them, or stagnating upon them, according to the greater or less rapidity of its course, and the obstacles it met with.

The twofold motion of the ancient ocean is noticed both by Buffon and Bergman, but neither of them have deduced from it the true explanation of the phenomena of which we here treat: Buffon attributes the formation of secondary mountains to deposition or sediments from the sea after the existence of fish, 1. *Epoques*, p. 143, in 8vo. which he says invested the bases of mountains without noticing any distinction of sides, p. 144 and 170. He thinks these sediments were equally conveyed from both poles towards the equator, for it is the equatorial regions that he thinks those mighty caverns opened, towards which the primitive ocean was impetuously borne and in which it was ingulphed, p. 181, 182, and 183. If so, similar declivities should be formed on the southern as on the northern sides of mountains, which is contrary to the observed facts. His explanation of the eastern and western declivities is defective and erroneous, for he attributes the abruptness of the western sides to the erosion of the coasts on that side (an erosion that exists only in fancy) and the smoothness of the eastern to the gradual desertion and retreat of the sea on that side, p. 184. and 185, a retreat equally fictitious, as De Luc has well shewn. Whereas since the general motion of the sea is from E. to W. if the erosion were of either side it should rather be on the eastern than on the western; besides, if the gentle declivities of the eastern sides of mountains arose from the gradual retreat of the sea, the petrifications of the secondary mountains thus formed should consist of such shell-fish as inhabit shallow seas or shores, whereas they consist chiefly of those called *pelagicæ*, which inhabit the greatest depths\*.

With respect to the eastern and western declivities, Mr. Bergman's account of the origin of their inequality agrees exactly with mine, 2. *Bergm. Erdeklotet* § 183 and 187, but he

\* 2. *Bergm. Erdekugel*, p. 315;

Remarks on the  
statements of  
Buffon and  
Bergmann.

fails in accounting for the inequality of the northern and southern, for he supposes the course of the water to tend equally from both poles towards the *Æquator* which would render the depositions equal on both sides, which is contrary to observation.

## XIV.

*A Memoir on Animal Cotton, or the Insect Fly-Carrier.* By M. BAUDRY DES LOZIERES, Member of several Academies, and Founder of the Society of Sciences and Arts, at Cape Francois. (American Transf. V.)

GENTLEMEN,

Preface.

BEFORE I enter upon the subject of this memoir, I ought to pay the tribute of praise which is due to your useful labours. But the style of eulogy is ill suited to the plainness of an American farmer, and while you are constantly employed in *deserving* praise, you cannot spare time to *hear* it.

Proposed new  
branch of West  
India commerce.

I am now going to communicate to you, with some observations upon it, a fact of entomology which I have myself witnessed during my residence at St. Domingo, and which, if I am not mistaken, deserves your greatest attention, because it may introduce a new branch of commerce with the West India colonies, and render very useful an animal which has hitherto been known only by the mischief which it occasions.

Every Inhabitant of the West Indies knows and dreads the greedy worm which devours their indigo and cassada plantations. But people have hitherto turned their attention more to the means of destroying it than of rendering it useful. It is indeed very natural to endeavour to destroy our enemy, but to compel him to be of service to us is by far the greater triumph.

Production,  
growth, and  
death of the  
cassada worm.

*Its Birth, Growth, and Death.*

The cassada worm is produced like the silk worm, that is to say, from the eggs which the mother scatters every where, after she has undergone her metamorphosis into a whitish butterfly, or of a light pearl colour.

The

The egg is hatched about the latter end of July. Its development is quick, for in September the worm is changed into a butterfly.

This month of September is the season of his loves. The constant motion of his wings shews the ardency of his passion which he indulges day and night and even while feeding. The excess of this indulgence soon destroys him, he dies in the same month after violent convulsions.

I have said that his life begins at the end of July. He is decked at his birth with a robe of the most brilliant variegated colours. This elegant livery, which nature seems to have delighted in forming, renders him always agreeable to the eye, which always dwells upon it with pleasure.

#### *Its Affinities.*

It has appeared to me to be a smooth caterpillar whose external shape is exactly like that of the silk worm. External Appearance, &c.

It differs however from it, by its size, by its thickness, and by the beauty of its colours.

It again differs from the silk worm, because it does not itself work the cone which I am going to speak of.

I leave it to the learned to delineate its external configuration, and to determine upon the family of insects to which it belongs. I shall only say that I do not believe it has, like the silk-worm, an intestine going in a direct line from the mouth to the anus, because it appears to me that this cause of elaboration would not have the same destination.

#### *Its Food.*

It feeds on cassada leaves, of which it is extremely greedy. Food. It feeds at all hours, day and night. It also nibbles the leaves of the potatoe, this is however but a transitory taste, it soon returns to the cassada leaf.

I have to observe that after it has taken its food, when the time of its metamorphosis arrives, it does not purge itself by diet, like the silk worm, but continues to eat to the last moment.

#### *The Approach of its Metamorphoses.*

In the month of August, and when on the point of undergoing its metamorphosis, it strips off its superb robe, and puts on Time and manner of its change.

on



on one of an admirable sea-green, this fundamental colour reflects all its various shades, according to the different undulations of the animal, and the different accidents of light.

*The Sting of the Ichneumon Fly.*

Immediate deposition of thousands of eggs in its body.

This new decoration is the signal of its tortures. Immediately a swarm of ichneumon flies assail it. I think I am not mistaken when I assert that there is not one of its pores that has not one of those flies fastened to it. There is even no necessity of making use of the microscope to see that he is covered with them.

In vain he struggles with all his might, raises himself upright to get rid of his cruel tormentors—He must submit. Those flies, of the smallest species, and which can only be studied by means of the microscope, drive their stings into the skin of their victim, over the whole extent of his back and sides. Afterwards, and all at the same time, they slip their eggs into the bottom of the wounds which they have made.

After having performed this dreadful operation, the ichneumon flies disappear, and the patient remains for an hour, in a drowsy and even motionless state, out of which he awakens to feed with his former voracity. Then he appears much larger, and his size increases every day. His green colour assumes a deeper hue, and the tints produced by the reflection of the light are more strongly marked. The animal in this state of factitious pregnancy, if I may so express myself, is worthy of all the attention of the observer of nature.

I shall not undertake the description of the ichneumon fly, it is well described in the books. If I have observed a difference, it is the same which exists between the European *gnat* and the *musquitoe* of hot regions, that is to say, that our West-India flies are of a lesser size.

I have now to describe the operation which the ichneumon flies, which are extremely small, perform at the very moment of their coming into the world; you will judge, gentlemen, whether this expression is accurate.

*Animal Cotton.*

Fibrous productions called animal cotton.

A fortnight after the ichneumon flies have thus cruelly deposited their eggs by perforating the unfortunate cassida-worm, that

that is to say, some time in the month of August, those eggs may be seen by the help of a microscope, hatching on the body of that animal.

Those eggs are all hatched at the same moment, and it is impossible to catch the moral point of time which may intervene between the birth of one and that of another. At one glance, the cassada-worm is seen covered with all the little worms that have just been hatched. They issue out of him at every pore, and that *animated robe* covers him so entirely, that nothing can be perceived but the top of his head. He then turns to a dirty white, the little worms appear black to the eye, but their true colour is a deep brown. History of the insects.

This operation lasts hardly more than an hour, and is followed by another which is not much larger but which is much more curious.

As soon as the worms are hatched, and without quitting the spot where the egg is which they have broke through, they yield a liquid gum, which by coming into contact with the air, becomes solid and slimy.

At the same time, and by a simultaneous motion, they raise themselves on their lower extremity, shake their heads and one half of their bodies, and swing themselves in every direction. Now is going to begin an operation which will afford the greatest delight to the admirer of nature.

Each of those *animalculæ* works himself a small and almost imperceptible cocoon in the shape of an egg, in which he wraps himself up. Thus, they make, as it were, their winding sheet. They seem to be born but to die.

Those millions and millions of cocoons, all close to each other, and the formation of which has not taken two hours, form a white robe in which the cassada-worm appears elegantly clothed. While they are thus decking him, he remains in a state of almost lethargic torpidity.

As soon as this covering is woven, and the little workmen who have made it have retired and hid themselves in their cells, the worm endeavours to rid himself of those barbarous guests, and of the robe which contains them, but he does not succeed in this attempt without the greatest efforts.

He comes out of this kind of enclosure, entirely flaccid and dull, instead of his former fat and shining appearance, his skin  
now

now appears flabby, wrinkled and dirty, and gives him the appearance of decrepitude. He is now an exhausted, suffering being, threatened with approaching death.

He will still gnaw a few leaves, but he no longer eats with that voracious appetite, which indicated an active and vigorous constitution. Shortly afterwards he passes to the state of a chrysalis, and after giving life to thousands of eggs, he suddenly loses his own, leaving to the cultivator who has not yet bethought himself of calculating the advantage that he may draw from him, an advantage which may be so improved as to much more than compensate the ravages which he occasions.

#### *Shell of the Ichneumon Fly.*

Shell of the  
ichneumon fly.

I had imagined that the thousands of little worms which this shell contains in the cocoons of which it is composed, would be hatched some day. I shut it up therefore in a box closed with great caution. Every morning, and very often in the course of the day, I examined it, in order to catch the moment when those little animals were to be born a second time.

In fact, at the expiration of about eight days, I found the inside of the box lined with a cloud of little flies. I made myself certain that they issued out of the little cocoon. Several which issued out of them before my eyes, left me no doubt as to the fact.

I then took up some of those flies, and putting them on a pincer, I examined them with a microscope.

They are bold and lively : they have four wings. Their antennæ are long and vibrating, their belly hangs by a very fine thread : there are some that have a tail, and others that do not shew it. Afterwards I satisfied myself that they feed upon small insects that appear to be of the family of *Acarus*. Those indications appeared to me sufficient to be satisfied that they belong to the family of the ichneumon.

#### *Observations on Animal Cotton.*

The animal  
cotton is white  
and pure ;

I have often held in my hand that cotton shell or wrapper. Its whiteness is dazzling. As soon as the flies have quitted the cocoon, it may be used without any preparatory precaution. It is made up of the purest and finest cotton.

I call



I call it *cotton* because it is *idio-electric* and is pervious to the electric fluid. conducts electricity;

I add to this denomination the epithet *animal*, in contradistinction to common cotton, which may henceforth be called *vegetable cotton*, so that the two species may be distinguished from each other by their names, as they are by their origin, although they are very nearly related to each other in their effects.

It is to be observed, that what might be called *cob-web* in the covering of the fly-carrier, or small flocks of silk which are probably intended to shelter the animal from the rain, is far superior to what is called *ferrit* before, and *fleet silk* after the preparation of the finer silk. There is no refuse, no inferior quality in animal-cotton. Every thing in it is as fine and beautiful as can be imagined. the whole is useful.

It is possible, if we may form a judgment by analogy, that medicine, which has extracted from silk what is called *English drops*, a remedy to which the greatest efficacy is attributed, may derive a similar advantage, perhaps for the cure of other disorders, from an extract of the animal cotton, which might be called the *St. Domingo drops*.

In short there is no need here of any of the precautions which the silk-worm requires. The robe which covers the fly-carrier, is worked every where, and every where perfectly well.

I shall only observe that as the rain speedily destroys the cassada-worm, the instant might be seized on when the ichneumon fly has deposited her eggs, to put the fly-carrier under shelter. His natural food might be procured for him, as is done with the silk-worm. Caution against rain.

The ichneumon fly never fails thus to come and deposit her eggs. I have never seen a fly-carrier that was not covered with the robe or shell that I have spoken of. I have continued this observation for many years, and the crop was so abundant, that I alone, could collect in less than two hours, the quantity of one hundred pints, French measure.

I repeat it, animal cotton is attended with none of the difficulties which occur in the preparation of vegetable cotton. It is so pure, that as soon as the ichneumons have left it, which happens eight or ten days after their reclusion, it may be carded and spun. Animal cotton judged to be much superior to the vegetable.

If

If it should want any preparation, it could be only in case it should not have been sufficiently guarded against dust and rain.

Vegetable cotton, besides the seeds that produce it and with which it is charged, is filled with extraneous matter, of which it cannot be freed, but with a minute attention, many hands and much time, or with the help of machines which have not yet been brought to perfection.

In every point of view, animal cotton appears to me to have a great superiority over that of the vegetable kind.

It will, perhaps, be wondered at, that experience has not long ago ascertained this fact, but let it be considered that the silk-worm and its use, were known long before any use was made of them, and that we are now carefully repairing the losses that we have suffered by the careless indifference of our fore-fathers.

The fly-carrier may experience the same fate, because it is less difficult to reason than to make experiments, but I dare hope that as soon as it shall have prevailed over the sophistry of indolence, it will stand the competition with silk and vegetable cotton. It is more abundant than either. It requires less time and less trouble to procure it.

This cotton  
makes good lint.

I have but one word more to add. Silk and vegetable cotton serve only to envenom and inflame wounds, which is attributed to the asperities of their filaments; I have frequently employed animal cotton as lint in the hospital of my plantation, it has always supplied the want of that made of flaxen linen, and I have not observed the smallest inconvenience to arise from the use that I have made of it.

Had it not been for the troubles that have laid our colony waste, and which have prevented the necessary communication, I should have brought to you a fly-carrier in every one of the periods of his life. You would have seen the eggs, the magnificent robe with which he is decked at his birth, the kind of food that he is fond of, the simple but noble vestment in which he wraps himself up on the approach of his tormentors, you would have seen those covering his whole body as it were with points, you would have seen him covered with his shell, and that same shell carded, spun and ready for the weaver. I had in a great degree already executed this design.

But

But it is too well known that I have not been able to save any thing in my flight from home, you will, however, be able at a future day to ascertain the truth of the fact that I have stated to you. I thought that a fact of this nature deserved to be deposited among your archives, and I may perhaps request of you the permission of depositing there some other still more curious facts.

Br. DES LOZIERES.

Philadelphia, Feb. 3, 1797.

## XV.

*An Essay on the Fecula of Green Plants. By Professor PROUST.*

HILAIRE ROUELLE is the first who discovered a substance in the green fecula analogous to the gluten of flour. This substance, since that time, has never appeared doubtful, because there are few chemists who have not seen its true characters. The fecula, of which it is the basis, in the opinion of Fourcroy, is only an imaginary being, or, at most, it has been too slightly examined to be admitted in the number of immediate vegetable products; he even proceeds so far as to suppose that the albumen, that animal product, which no one ever before suspected to be contained in plants, is the substance which ought to be admitted instead of the glutinous part of green feculæ.

Opinion of Fourcroy that the fecula of plants contain albumen and not gluten, as Rouelle asserted.

Are albumen and gluten found together or separate in the juice of plants? This is the question I have proposed to solve, and I shall endeavour to resolve it in this part of my observations on his "System of Chemical Knowledge."

are both found in plants?

To save my readers the trouble of turning to the work, I shall copy the passage in which the author has collected the facts and arguments upon which he has formed his judgment. This passage is still more remarkable for the difference it presents between his manner of characterising other vegetable products, and that of the chemists of the present day.

"Rouelle the younger, who examined and particularly compared it with animal matters, asserts that he has found it in the coloured feculæ, and especially in that which is called the green fecula of plants. But the name of fecula being

Quotation from Fourcroy.



given indifferently to the fibrous matter contained in the juice of plants, and to the starch, has induced chemists to consider the latter as part of the residue of solid vegetable substances, and there is reason to think that it was only by analogy, or perhaps by certain equivocal properties, that Rouelle believed the green matter contained any of the glutinous substances; subsequent experiments at least, and such as I have many times repeated on coloured feculæ, have not yielded me the confirmation of this assertion, nor has any thing been ascertained with certainty, that the gluten is one of the principles of this latter fecula."

The name of fecula, says Fourcroy, being given *indifferently* to the fibrous matter contained in the juice of plants, and to the starch, has induced chemists to consider the latter as part of the residue of solid vegetable substances, and there is reason, &c.

He accuses the ancient chemists of inaccuracy,

I shall first notice that this opinion is not correct. For example, the chemists of the present day will never agree with Fourcroy, that the confusion arising from the improper application of terms, of which later chemists have justly complained, has, by a necessary consequence, produced inaccuracy in those who have preceded us. Our masters, it is true, gave bad names to things, but they did not confound them more than we do.

which is not well founded.

Even at the time when every vegetable precipitate was called a fecula, the similarity of terms never misled them so far as to cause them to confound the starch with the residue of the solid parts of plants. First, we are not acquainted with any residue of this description, to which chemists can reasonably compare it; and secondly, if any of them did take the green fecula for a residue, there was not one of them who

did not perfectly know the difference between this fecula or residue and starch; and as no such confusion is to be found in their works, the reproach is unjust; we need only look into those of Rouelle, Macquer, Baumé, Sage, Parmentier, &c. to be convinced that the term of fecula has never misled these authors into assimilations so discreditable to their judgment, as to place in the same rank the green fecula, the residue of the solid parts, and the starch.

The fecula was never confounded with fibrous matter.

We will now proceed to the green feculæ, and affirm, that in the laboratories in pharmacy, and still less in the hands of a chemist so celebrated for accuracy of observation as Rouelle,

the

the broken fibres of green plants have never been confounded with that beautiful soft liquid expressed from their leaves, or with that emulsive product which passes pure through the strainer, and which, by its excessive fineness and the brilliancy of its colour, differs so very much from the herbaceous filaments.

Again, if it were true that the fecula were a body homogeneous with the rest of the plant; if it were possible to consider it as in no respect different from the other parts, but by being more bruised, would it not be possible, by completing the trituration of the remainder, to convert it also into fecula? When a fresh plant is bruised by the pestle, it is broken small; its texture is destroyed, but it is not pulverized.

Its state of diffusion in water is not from grinding.

This crushing for a few moments, differs too much from a dry pulverization, to admit of any comparison between the fecula so produced, and a moistened powder. When an aqueous juicy plant, a sedum for example, is crushed with a roller on the slab, its expressed juice will afford fecula. Certainly it is not to the trituration, that a fecula is indebted for its softness, its fineness, and the impalpability which distinguishes it from powders. It is molecular in its own nature, and is even, perhaps, crystallized in those fibrous cavities where it is deposited by vegetation.

Rouelle asserts, according to Fourcroy, that the feculæ contains a principle similar to animal matters, &c. Rouelle does more than that: little contented with simple assertions, he proves it, not by *analogy* or *equivocal properties*, but by a succession of convincing facts, by approximations which have been universally admitted, because they combine together the most prominent characters which were then known, or are even yet known, to exist in animal substances. Whence otherwise could Rouelle have drawn his analogies, to enable him to compare, as he does, the green fecula with the gluten of wheat? In fact, what is there in the common appearance of these two products that can lead to the comparison? Their points of comparison must be sought for in their composition, in their chemical properties, and this was done by that indefatigable chemist. These are approximations drawn from analysis which serve as the basis of the Memoir he has written on the green feculæ, and of which there is no mention in the System of Chemical Knowledge; doubtless because in the

The doctrines of Rouelle were not mere assertions, but clear investigations from numerous facts.

which Fourcroy has not detailed.



opinion of its illustrious author, Rouelle had confounded albumen with the gluten, and the detail of his supposed mistake was considered as a matter of indifference in the history of chemistry.

Rouelle first discovered the peculiar animalized substance of the green fecula,

Rouelle however found in the fecula of sorrel, a product so amply possessing the chemical properties of albumen, that he particularly insisted upon it in order to fix the attention of his time upon a substance so animalized; and as he afterwards obtained it from a plant, which, according to Fourcroy, does not yield the slightest trace of albumen, it is now incontestable, as it was then, that Rouelle was the first who discovered in the juices and green feculæ a *product* which, if not intitled to the name of albumen, possesses, nevertheless so strongly, all the properties by which the attention of chemists has been called exclusively to it, that it is no less proper to be urged in the history of their discoveries, than albumen itself.

and the identity of veg. gluten and cheese.

It is to the same penetrating eye, the same impulse of genius which led him to anticipate discovery, that he is indebted for that of the astonishing resemblance between caseum and gluten, when they have both undergone that species of fermentation which transforms them into the cellular combination, the odorous and savory compound, called cheese. And the gluten, in this singular result, resembles the other more completely the more carefully it has been washed. Macquer, when he published what continues to be every where repeated, that part of these changes were occasioned by some remains of starch, was incorrect in his notion. Starch, a substance always inactive in fermentation, in that of bread, of beer, and even in germination, could only retard the effect produced by the gluten itself, and consequently could only destroy, in part, those characters, from which Rouelle stated the resemblance of these two products.

Peculiar fermentation of cheese.

And even their analysis extends far beyond the limits assigned to them; for when the gluten has changed its insipid and viscous mucosity for the cheesy state; when it has gone through all the stages of that fermentation which is essential to that condition, it is found also to have acquired the taste of those sharp and burning salts which constitute the principal merit of the Roquefort cheese; salts which have nothing in common with what is added, but are found equally powerful in the curd which has been washed and left to its own fermentation.



In fact in the cheese from gluten, as well as in that from Salts generated in cheese, &c. animals, potash and sulphuric acid enable us to find that ammonia, and that vinegar discovered by Vauquelin. Is the ammoniacal acetite then one of the ingredients which gives the flavour to cheese? I only know that alcohol applied to strong cheese deprives it of all its taste. An analysis of that species might afford curious results; but let us return to the green feculæ; examine them by the lights of modern chemistry, and endeavour more particularly to discover whether albumen actually exists where Beccari and Rouelle found gluten.

### *Green Fecula.*

I. The fecula undergoes, by heat, a change capable alone of giving it a decisive character as to its nature. I mean that The green fecula very con-  
concretescibility which has so few examples in vegetable products; that agglutination which compresses the particles together, and produces the appearance of a cheesy curd. Though crefible by  
the fecula, before this change, passed easily through the heat.  
strainer, it can no longer do so after having been heated; a  
peculiar hardening has deprived it of its tenuity; but heat does  
not coagulate the fibrous tissue; in this respect the fecula does  
not resemble the broken straw of green plants.

II. The fecula, separated from the juice by filtration, acquires an elastic and horny consistence by drying. It is soft-  
Elastic and  
horny when  
dry.  
tened with difficulty in heated water, but will not become soft  
even at the end of a month; notwithstanding it is moistened it  
still retains its horny state. When bended it will return to its  
shape, and cannot by any means be crumbled: these quali-  
ties also are not found in the dry woody pulp.

The feculæ of green and white cabbages, cresses, hemlock, &c. do not lose the property of coagulating by heat from that  
More concrefci-  
ble than white  
of egg;  
cause. Into water heated to between 50 and 60 degrees,  
plunge two matrasses of equal size, one containing diluted  
fecula, and the other with the water and white of an egg;  
the fecula will harden and be collected in flocks, such as are  
formed in a juice which is clarified by heat; but at this tem-  
perature the albumen will not even lose its transparency.

III. The green fecula is very nearly in equipoise with water, for that of plants which are not acid, frequently requires  
a week to precipitate.

Put

precipitable  
from water by  
alcohol and by  
acids.

Put some fecula washed and diluted into three glasses of equal size. To the first add a little alcohol, a few drops of acid to the second, and place the third between them for comparison. In the two first the precipitation will be complete in half an hour, while in the third it will be hardly begun; alcohol and the acids therefore coagulate feculæ; but they have no such action upon the woody residue.

Alcohol takes  
up near one sixth  
of resinous mat-  
ter.

IV. One hundred parts of the dried fecula of hemlock yielded to alcohol from 15 to 16 of green resin. After repeated infusions to which it was subjected, it remained of an earthy grey, and the alcohol was incapable of bleaching it. Sage, who was well acquainted with feculæ, found that they yielded about one third of their weight in resin; in order to separate it easily, the fecula must be thrown into spirits of wine while yet moist, the fluid then penetrates and attacks all its parts; but the effect is much more difficult when it has become horny by drying.

(To be continued.)

## SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

### Prize Questions of Foreign Learned Societies.

National Insti-  
tute. Prize  
questions.

THE National Institute of France held a public sitting on the 20th Vendemiaire, when the new subjects for prizes were announced.

The *Class of Mathematical and Physical Knowledge* proposed the following question:

Winter sleep of  
animals.

“To determine, by anatomical and chemical observations and experiments, what are the phenomena of inactivity which certain animals, such as marmots, dormice, &c. undergo in the winter, with regard to the circulation of the blood, the respiration and the irritability; to inquire what are the causes of their sleeping, and why it is peculiar to these animals.”

This question was proposed before, and the prize was to have been decreed in this sitting; but the meeting being of opinion that the memoirs received did not contain sufficient information, decreed that it should be proposed again, and that the prize should be doubled. It will be of the value of two kilogrammes of gold (about 6.800 fr.); and will be distributed

tributed at the public sitting of Vendemiaire in the year 13. Memoirs must be transmitted to the Secretary of the Institute before the 15th Messidor of the year 12.

The question proposed by the *Class of Moral and Political* knowledge is,

“ To determine how the faculty of thought can be decom- Decomposition  
posed; and what are the elementary faculties which can be of thought.  
discovered in it?”

The prize is a gold metal of the weight of five hectogrammes (about 1700 fr.): it will be adjudged at the public sitting of Germinal in the 12th year of the Republic.

The works cannot be received after the 15th Nivose of the same year.

*Prize for Geography.* “ To compare the geographical Geography  
charts by Ptolemy, of the interior of Africa, with those which prize.  
have been transmitted to us by subsequent geographers and historians, with the exception of Egypt and the coasts of Barbary from Tunis to Morocco.”

This subject had been proposed in the year 9, but the memoirs sent not corresponding to the conditions of the notice, it was renewed.

The prize is a gold medal weighing five hectogrammes (about 1700 fr.), and will be adjudged in the public sitting of Messidor in the year 12. The memoirs must be sent in before the 15th Germinal of the same year.

The Medical Society at Montpellier has proposed two Med. Soc. of  
prizes of 500 francs each, of which the first is to be adjudged Montpellier.  
in May 1803, and the second in May 1804, on the following Prize questions.  
questions:

1st. In what kinds of diseases, and under what circum- Inflammation?  
stances, is inflammation favourable or dangerous? and, in the  
treatment of such diseases, in what cases ought it to be ex-  
cited or checked.

2d. To ascertain by experiment and observation, what de- Medicines by  
gree of confidence can be placed on the use of certain sub- friction?  
stances, by friction, which are generally administered inter-  
nally; to determine the effect of such remedies, in both me-  
thods, and also the quantity of the dose; to point out the dis-  
eases and cases in which one method is to be preferred to the  
other; and finally, to determine, in the different maladies,  
to what parts of the body the application of these remedies,  
are most efficacious.



*Flash from an Air-Gun.*

Luminous flash  
from an air gun.

M. Pictet, in a letter from Paris of the 1st of January last to Mr. Tilloch (*Philos. Magazine*, Vol. XIV. 363.) states a communication to the National Institute of France on the 29th of December by M. Mollet, of Lyons, respecting the luminous appearance produced by the discharge of an air-gun in the dark, a phenomenon which he considers as never having been before observed. It has, however, been known for some time in this country, having been first mentioned, I believe, near a year and a half ago by Mr. Fletcher at a meeting for philosophical experiments and conversations, which was then held weekly at my house. Several discussions accordingly took place with regard to its cause,—as, whether it was produced by electricity, or the change of capacity in the expanding fluid, &c. and it was intended to institute a course of experiments on the subject, but some other objects of enquiry intervening the matter was postponed. It is a curious phenomenon, and deserves investigation.

*Letter from Professor PROUST to J. C. DELAMETHERIE.**On the Sugar of Grapes, and the Compound Nature of Urée.*

New sugar in  
the grape.

I have discovered a new sugar in the grape, which is the basis of wine; it is different from that of the sugar-cane, crystallises differently, &c. It is contained in the proportion of about 30 per cent. in the juice of the grape. Azote is uniformly combined with the carbonic acid in the fermentation of wine: in that of gluten it is pure hidrogen, which is disengaged with the carbonic acid.

The urée is a  
compound con-  
taining ammo-  
nia.

Tell Vauquelin that the urée, in the state in which they have examined it, is a saline substance saturated with ammonia, and not a simple product; he need only apply sulphuric acid to carry off this ammonia, and to have the urée pure, but coloured with a resin from which I have not yet been able to free it.

*J. de Physique.*

*On*

*On the Use of Sulphate of Soda in the Manufactory of Glafs.*

By PAJET-DESCHARMES \*.

Since the notice which was given in the *Journal de Physique* that sulphate of soda might be used, without preparation or any intermediate agent, in the fabrication of siliceous glafs, Cit. Pajot-Descharmes has thought fit to publish the result of his principal experiments on the employment of this salt in glafs-making.

Use of sulphate  
of soda in glafs  
making.

It appears from the experiments which this operator has reported in the *Journal de Physique*,

1. That sulphate of soda and sand alone, in various proportions cannot succeed.

2. That sulphate of soda, mixed with pounded charcoal, in the proportion of a tenth or twentieth part, yielded a yellow glafs more or less black, of the nature of obsidian pastes or stones: the crucibles were then very slightly acted upon.

3. That equal parts of carbonate of lime, dried sulphate of soda, and sand, produced a beautiful glafs, clear, solid, and of a pale yellow: the crucibles were then very little corroded.

Cit. Pajot-Descharmes observes, that notwithstanding the pains he took, he could never obtain a glafs with sulphate of soda that was not of a yellowish green, whereas muriate of soda (sea-salt), treated in the same manner as sulphate of soda, always produced a glafs of a light blue tinge inclining more or less to green.

Cit. Pajot-Descharmes proposes to treat more fully, in a particular memoir, on all the experiments he has made on this subject.

*Observations on the Necessity of immersing Seeds in Water in Times of Drought.* By Ant. ALEXIS CADET-DE-VAUX.†

Franconville-la-Garenne, 2 Brumaire (Oct. 23.)

To reap, we must sow; and, from the drought which has prevailed for six months, sowing is not easy, for our gardens

In dry seasons  
the seed grain is  
in danger of pe-  
rishing in the  
ground.

\* Abridged with remarks in the *Journal des Mines*, No. 69, from a Letter from Cit. Pajot-Descharmes, to J. C. de la Metherie, in the *Journ. de Physique*, Vol. LII. p. 210.

† Decade Philosophique, No. 4. An. XI.

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are no longer cultivated with the spade and the harrow, but with the mattock and the pick-axe; the plough, however, on account of its strength, can still be employed to till the ground, except in compact lands, and those which are stiffly bound. But it is not enough that the land shall have been tilled to enable us to sow it, the seed must also germinate, without which it dries and perishes, or becomes the food of animals and insects. For without rain, or dew which moistens at least the surface of the soil, there can be no germination. In the mean time the season advances, and the seed-time is already late. Let us point out, then, to the husbandman, a method of preventing the inconvenience of drought; it is, not to commit his seed to the earth, until it is impregnated with the moisture necessary for its germination.

Remedy; to  
soak it first in  
water.

Experimental  
proof.

We may refer to a great example: the Chinese do not deposit a single seed in the earth until it has been immersed in water. And I will adduce an experiment nearer home, which is in favour of this practice. Five years ago, I sowed half an acre (*arpent*) of land with wild succory, lucern, and pimpernel. With a view to compare the produce of these plants, I sowed them in rows two feet asunder. The spring was very dry, and I soaked each of the seeds in water for forty eight hours. The quantity soaked was not sufficient; for there was required as much more as to sow nine rows, three of each kind. I took advantage of this circumstance to compare the effect arising from the seed being prepared or not prepared by immersion. The result was, that in the nine rows sowed with the dry seed, not more than thirty plants came up in five months: while the remainder of the land was covered, and formed a most beautiful artificial meadow. The rain fell too late to sow these nine rows, and it was necessary to sow them again in autumn.

Let us now apply these facts to the sowing of corn.

Liming recom-  
mended; by im-  
mersion.

The husbandman is in the habit of liming his wheat when he apprehends the rot. This year he has no need to dread that evil; nevertheless let him use the lime, but by immersion; for in general the method of application is defective. The operation is usually confined to a simple sprinkling of the heap of wheat with lime water, while it is turned over with a shovel.

The



The good, the only way of liming, is by immersion; put the seed into tubs, and cover it to the height of four or five fingers breadths with lime-water, made so hot that the hand cannot be kept in it without difficulty; cover it up, stirring it three or four times in the twenty-four hours; after which draw out the bung that the water may run off, the quantity will be but small; it will be nearly all absorbed by the grain, which must be taken from the tubs, spread out in the air and then sowed.

Twelve bushels of wheat, immersed for twenty-four hours, will absorb nearly one-fourth of water, that is to say, they will swell to fifteen or sixteen bushels by measure. Let us now investigate the theory. Every grain of this corn carries with it to the earth a quantity of water, more than sufficient to ensure its germination. This water acts principally upon the extractive matter of the husk; it dissolves this principle, one of the properties of which is to attract and strongly to retain moisture. Hence this water will not be evaporated. If instead of pure water, we use dunghill water, which is saturated with this extractive matter, together with deliquescent salts, and fatty matter, then the most minute quantities of surrounding humidity will be attracted towards the grain. But in truth it will, after this treatment, succeed very well without the speedy assistance of the rains and dews; it possesses a sufficiency of moisture to put forth its germ, to throw out its radical, and in short, to secure its germination. The grain which has been steeped gains, in ordinary seasons, from twelve to sixteen days in advance before that which has not been steeped; and in times of excessive drought, it gains every thing. If steeped, it germinates and grows; and if not steeped it dries and perishes. Let the rains come, let them continue, still I advise immersion; which by forwarding the germination, remedies the inconveniencies of a late seed-time.

Method described.

Wheat becomes one fourth larger by absorbing water.

Dunghill water is best.

Recapitulation.

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*Extract from a Memoir by CIT. FOURCROY, on the Chemical Nature of Ants, and on the simultaneous Existence of two Vegetable Acids in these Insects.*

Samuel Fischer first described this acid in 1670. It has since been more particularly examined by Margraff, Arvidson, Bergman, &c. and finally by Cit. Deyeux, who established the identity, which Margraff had previously suspected

Experiments shewing the existence of malic as well as formic acid in ants.

to

Experiments  
shewing the ex-  
istence of malic  
as well as formic  
acid in ants.

to exist between the formic and acetous acids. Nevertheless there were still some doubts to remove, and it was these which induced the Citizens Fourcroy and Vauquelin to undertake the following investigations :

Some red ants (*formica rufa*, Lin.) were crushed in a marble mortar. A sharp vapour was disengaged, similar to that of radical vinegar; and the alcohol, in which the ants were put to macerate, was tinged yellow.

This infusion produced, by distillation, an inflammable liquor, slightly acid. At the same time it formed a brownish sediment, which was carefully separated. This sediment became covered with an acid liquor, which was saturated with lime.

The latter combination became brown and thick : it had a pungent nauseous taste, and the air produced bubbles in it, as in soap-suds.

One part of this compound, mixed with one part and a half of sulphuric acid and two of water, produced a very thick magma, which, by distillation, yielded an acid liquor, without colour, of an empyreumatic odour, but which did not discover any trace of sulphuric acid.

This acid combined with potash, formed a true acetite.

The brownish thick compound, of which we have spoken above, formed, by solution in acetite of lead, an abundant precipitate, which proves that the acid extracted from the ants by the alcohol, contained something beside the acetous acid.

The same calcareous compound mixed with a solution of nitrate of lead, yielded an abundant yellow precipitate, which, treated with sulphuric acid weakened by water, formed a new precipitate, heavier and whiter. The supernatant fluid was slightly acid and sugary : it precipitated abundantly the nitrates of mercury, of silver, and of lead.

Many other facts, in addition to those we have mentioned, are sufficient proofs that the malic acid is joined with acetic acid in the liquor which is extracted from the ants by the alcohol; and it is doubtless the presence of this acid which has led those chemists into an error, who formerly treated of this subject.

The ants, after having been separated from the alcohol, yielded, by distillation, an empyreumatic fetid oil, carbonate  
of

of ammonia and acetite of ammonia, the whole dissolved in a great quantity of water.

The brown substance left from the distillation of the infusion in alcohol, was insoluble in water, and soluble in alcohol, except a small quantity of a brownish matter, which appeared to the authors to be albumen. The solution of this brown substance in alcohol became milky by the addition of water; and, in a few days, deposited a precipitate of a resinous appearance, which seemed to be a fat matter of a peculiar nature.

Lastly, the residuum from the ants was an animal carbon, which, after combustion, left only phosphate of lime.

The memoir finishes with observations on the presence of the acetous and malic acids in ants in particular, and in organic bodies in general.

*Bulletin des Sciences, No. 70.*

*An Essay on the Relation between the specific Gravities and the Strengths and Values of Spirituous Liquors; with Rules for the Adaptation of Mr. Gilpin's Tables to the present Standard, and two New Tables for finding the Per-centage and Concentration when the specific Gravity and Temperature are given. By ATKINS and Co. Mathematical Instrument Makers. Quarto, 74 Pages. London, Cadell and Davies, and Robinsons, 1803.*

It is certainly very remarkable, that no mode of denomination by which the real strength of spirituous liquors could be defined with any tolerable degree of correctness, has ever yet been in general use in this or any other country. The mischiefs accruing from the want of such a system, and the disputes which were daily arising between the revenue officers and the traders, with respect to these matters, induced the legislature several years since to pass an act by which was declared, that all spirits should be deemed and taken to be of the degree of strength which should be denoted by Clarke's hydrometer. The inconveniences, however, which resulted both from the complex construction of the instrument itself, and the ineligibility of the mode by which the strengths of these liquors are denominated, according to the system proposed by its inventor, having been long felt, and both one and

Atkins on the specific gravities of spirituous liquors.



Atkins on the  
specific gravities  
of spirituous  
liquors.

and the other having accordingly in a great measure fallen into disuse amongst all but those immediately concerned in the collection of the duties, the lords of the treasury during the last session of parliament, applied for and obtained an act, empowering them to order any other hydrometer to be used for the purposes of the revenue, which might be found more conveniently applicable to them. This has of course rendered a review of the whole matter indispensably necessary, and the authors of the pamphlet before us, whose attention to this subject is already sufficiently known, have accordingly with great perspicuity traced the principles on which alone an equitable system can be established.

After shewing the connection between the subject of the present tract, and the appreciation of our weights and measures in general, they proceed in the preface to state the authorities on which they have founded their estimation of the weights of the known measures of distilled water. These are principally the experiments of Sir George Shuckburgh Evelyn \*, and those of the French commissioners of weights and measures. From the former it appears to result, that the cubic inch of distilled water at 60° of Fahrenheit's thermometer, weighed in air at the same temperature when the barometer stands at  $29\frac{1}{2}$  inches, is equal to 252.506 troy grains, and from the latter, equal to 252.55, so that the authors take it to be under the circumstances sufficiently near  $252\frac{1}{2}$ , and that this weight therefore corresponds with the specific gravity of 1,000. Their appreciation of the cubic measure of the wine gallon is similar to that of the board of excise, viz. that it contains 231. cubic inches, and they estimate the pound avoirdupoise at 7,000 grains troy. The preliminaries and a detail of the calculations and reasons on which the deductions are founded, are very properly introduced in this place, as necessary to the understanding of their subsequent estimation of the specific gravity of proof spirit.

The work itself treats of the subject under the following general heads; of the general relation between the specific gravities and the strengths and values of spirituous liquors, and the circumstances by which the former are influenced; of the

\* See Phil. Journal, Quarto Series, Vol. iii. p. 97, &c. and Octavo Series, Vol. iv. p. 35. (No. for January last.)

standard of proof; of over-proofs and under-proofs, and the modes of appreciating them; of Mr. Gilpin's experiments and tables; and of the means of adapting them to the present standard, with various problems and rules for that purpose; and concludes with the two tables of the authors mentioned in the title-page.

The first chapter treats very perspicuously of the subject in general, and the intricacy in which it is involved by the joint effects of concentration and change of temperature.

In the second the authors proceed to the estimation of the strength of proof spirit, as deducible from a loose passage in an act of parliament passed in 1762, by which it is enacted, that "for the purposes of that act, each gallon of brandy or spirits of the strength of one to six under hydrometer-proof, shall be taken and reckoned at 7 lbs. 13 oz. the gallon; and which seems to be the only clause in the statute-book, in which the strength of any kind of spirituous liquor is attempted to be defined by reference to its specific gravity. The temperature of the liquor and some other circumstances are here left to be assumed, which the authors have accordingly been under the necessity of doing, and from hence they deduce, that proof spirit may reasonably be considered as having the specific gravity of 920 at 60°. according to the common acceptance; and this they accordingly recommend as the future legal definition of that standard.

The third chapter contains a distinct account of the various modes of comparison hitherto in use, and of their respective inconveniences and defects. According to the system proposed by the authors, the denomination itself would at once indicate the real comparative value of the spirit in question, by reference to the equivalent quantity of proof spirit, or that which would produce or be producible from 100 gallons of the former, by the addition of water to the stronger of the two, till they were reduced to the same degree of strength; allowance being made in all cases for the concentration which takes place by mixture, and the change in bulk and consequent difference in value by measure, according to the existing temperature. Thus, an under-proof of which 100 gallons might be made up with water from 80 gallons of proof, would be called spirit of "80" or "20 under-proof," and a spirit of which 100 gallons would when reduced to proof, make 134, would be called "134,

or

Atkins on the  
specific gravities  
of spirituous  
liquors.

Atkins on the  
specific gravities  
of spirituous  
liquors.

or "34 over-proof." We here find some arithmetical rules introduced for the purpose of illustrating the practical facilities resulting from the use of such a system, and the chapter concludes with a section on the general construction of the hydrometer, in which, however, the authors have modestly forbore to speak particularly of those of their own construction, which are already so well known to the public.

In the fourth chapter, which treats of the experiments and tables of Mr. Gilpin, the author speaks with its merited praise of this great work, and in the fifth they give a variety of rules, some of them very curious, for solving all the most useful problems respecting this subject from these tables, which they consider as an authentic register of experimental results. These processes are all founded on the supposition that proof spirit is of the specific gravity of 920 at 60°.

The sixth and last chapter contains the two tables of the authors already mentioned, with their use and application. These tables in a great measure answer all the purposes of the more voluminous ones of Mr. Gilpin, from which they are calculated by the preceding formulæ, and that in a much simpler and easier manner, being adapted to the present standard of proof, and giving the required per-centage and concentration by mere inspection.

Having thus stated the outlines of this work, it would be to take notice of a circumstance of minor consequence if I remarked, that it is a very neat specimen of typographical elegance. The subject is a most important one, and the philosophical reader will be pleased to find it here treated in a manner which renders it no less worthy of the attention of the man of science, than of those whose commercial interests are involved in the event of the present investigation.

A. B.



# Sliding Stop-cock for pneumatic Experiments

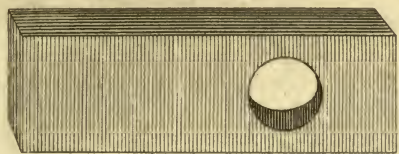


Fig. 3.



Fig. 2.

Fig 1

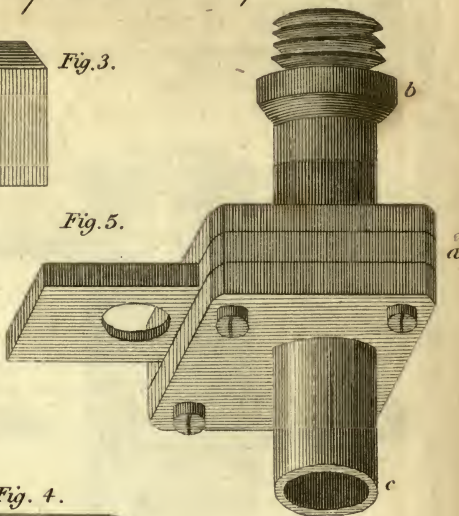
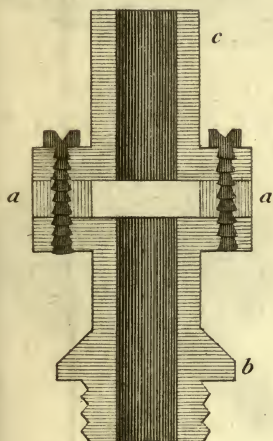


Fig. 5.

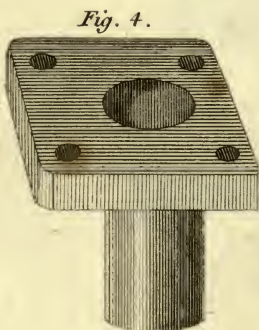


Fig. 4.

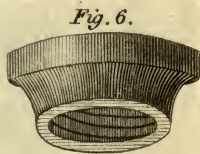


Fig. 6.

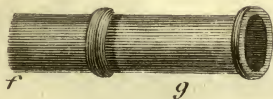


Fig. 7.

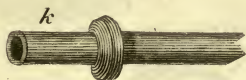
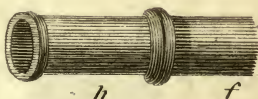


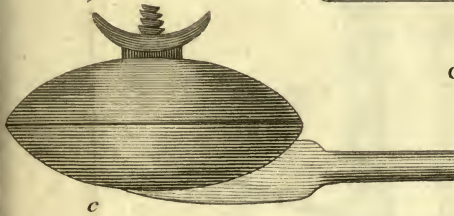
Fig. 8.



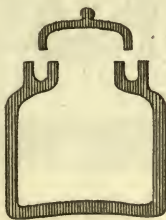
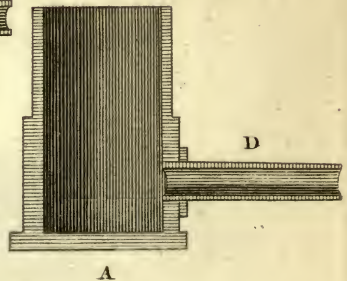
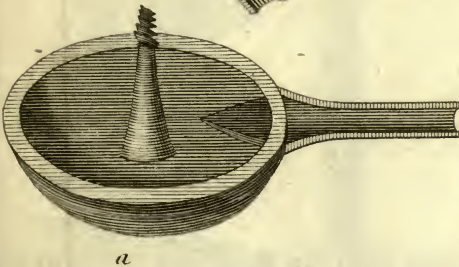
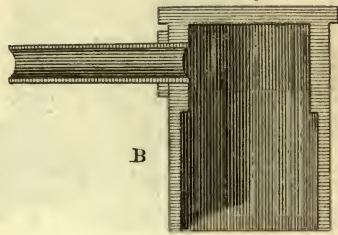
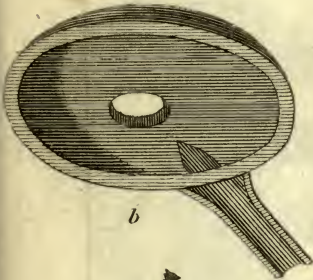
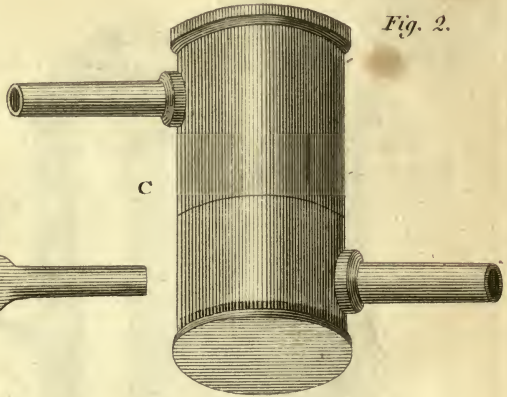


# *Metallic Joints for pneumatic Apparatus*

*Fig. 1.*



*Fig. 2.*

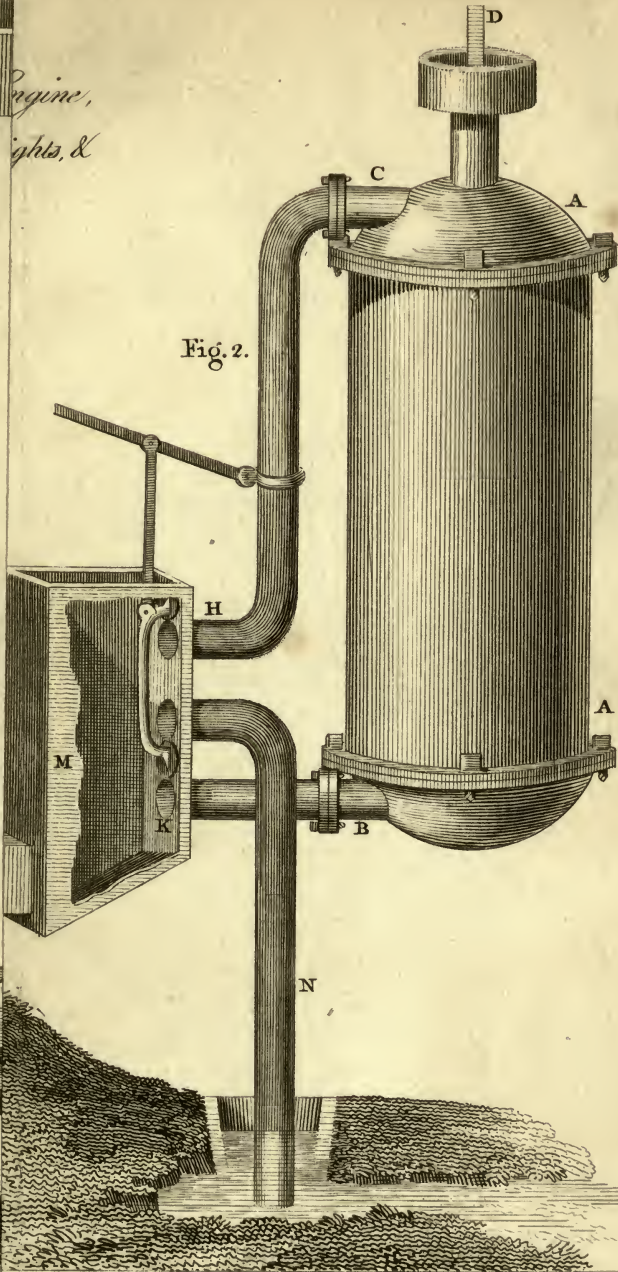






Engine,  
lights, &

Fig. 2.

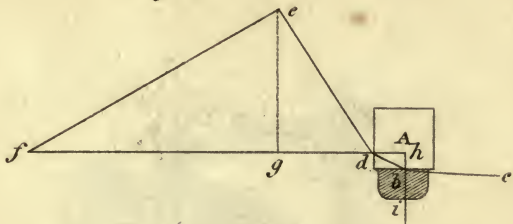




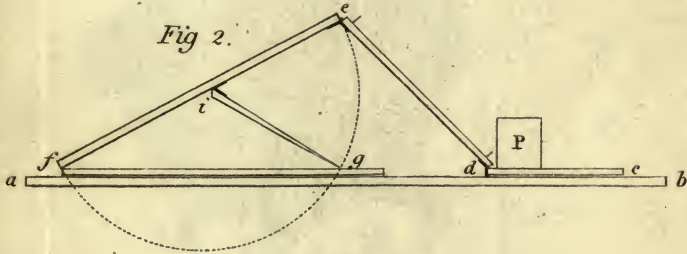


*D.<sup>r</sup> Collaston's Method of examining the  
refractive & dispersive powers of Bodies.*

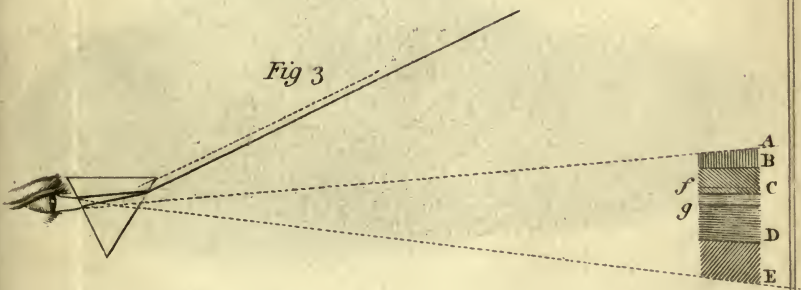
*Fig 1.*



*Fig 2.*

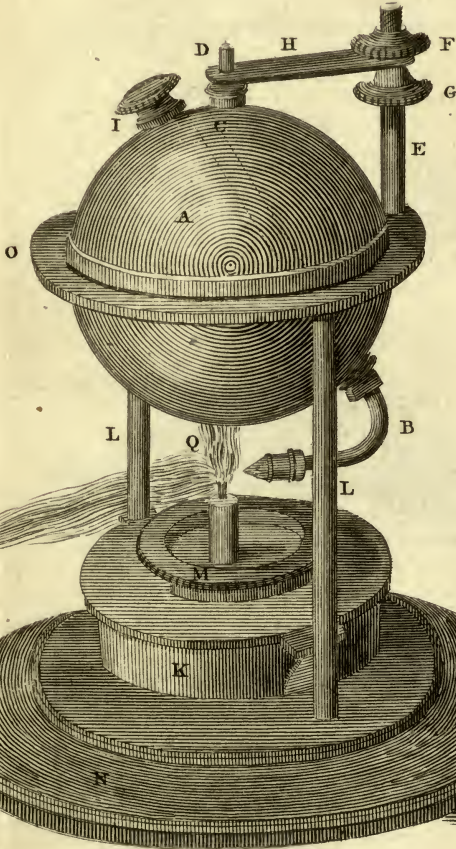


*Fig 3*





*Blow-Pipe, by Alcohol. By M<sup>r</sup>. Hooke.*







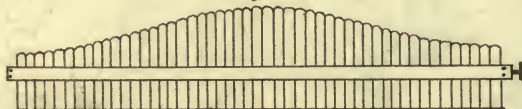
*Fig. 6.*

*Dr. Young's Harmonic Sliders.*

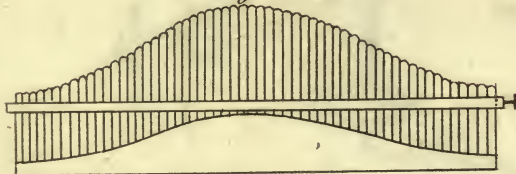
*Fig. 1.*



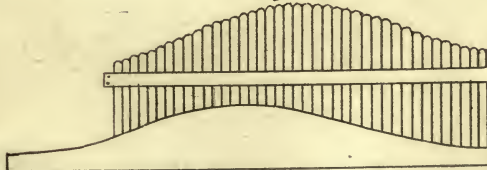
*Fig. 2.*



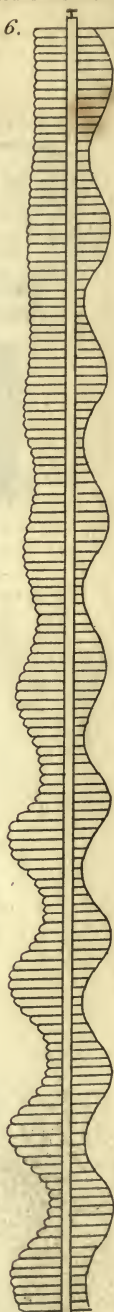
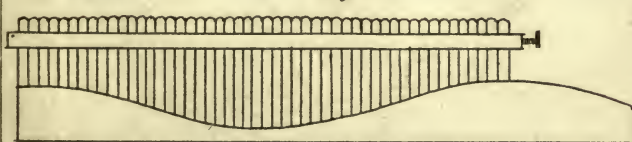
*Fig. 3.*



*Fig. 4.*



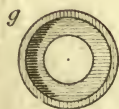
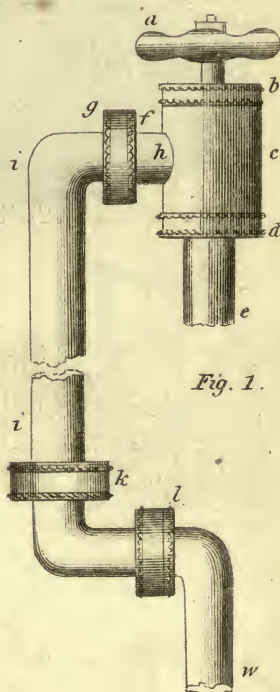
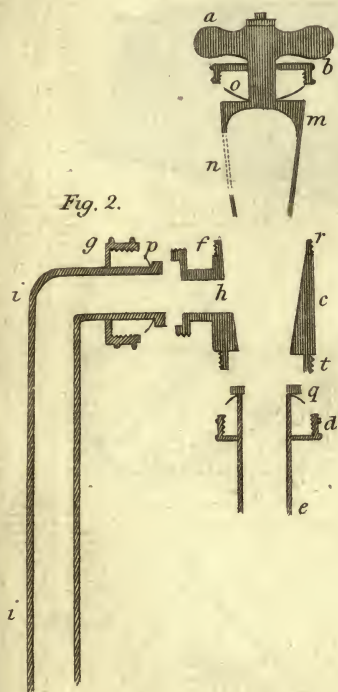
*Fig. 5.*





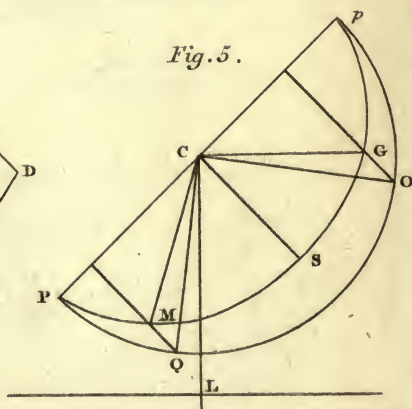
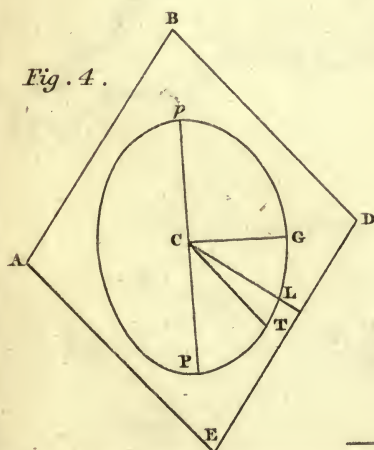
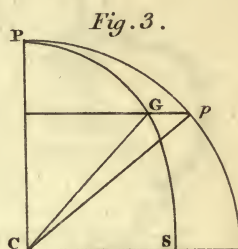
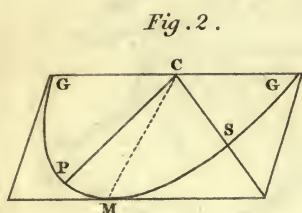
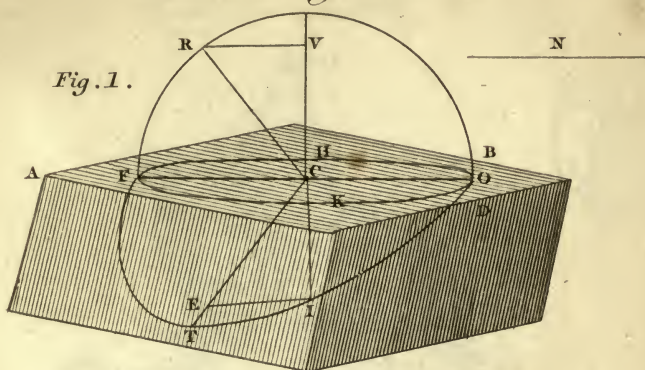


*Steam-cocks, Joints & Tubes, W. N.*





*Dr. Wollaston, on the oblique refraction of  
Ice-land Crystal.*



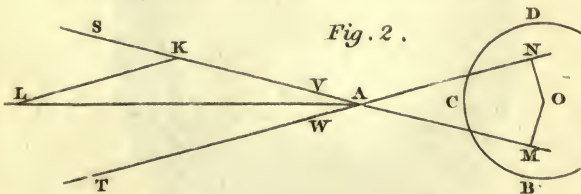




*Mr Dalton's Sketch of the Rivers in England & Wales,  
divided into districts.*



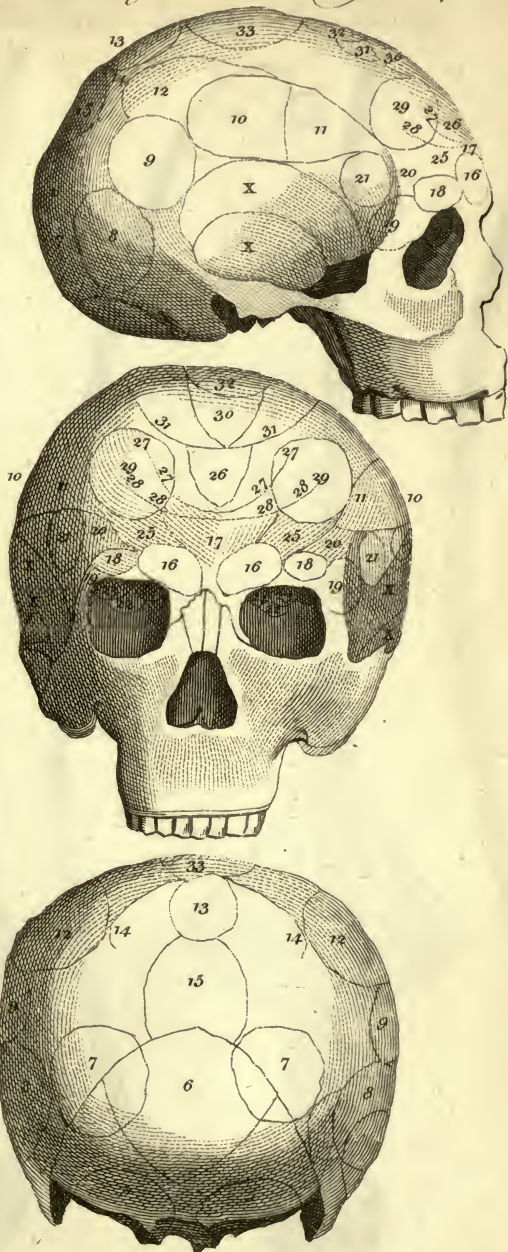
*Mr Gough, on sonorous Undulation.*





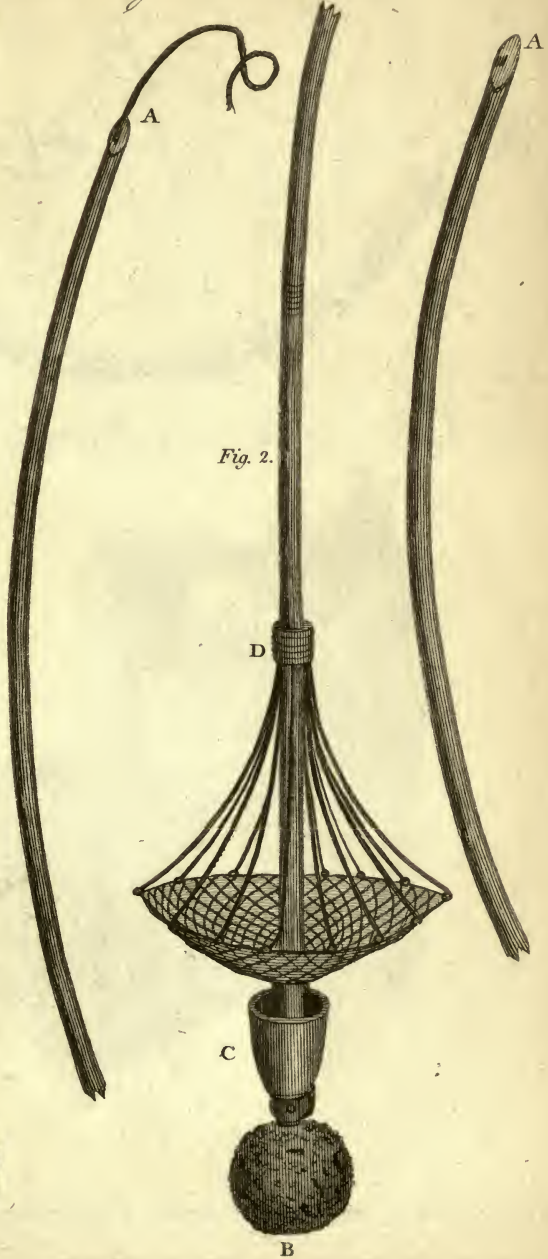
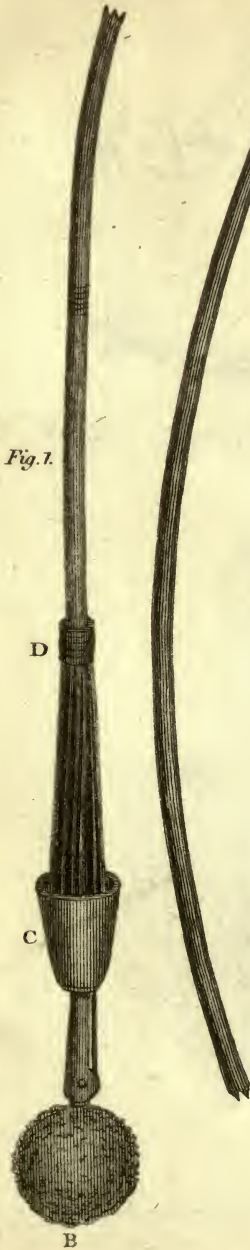


*The craniognomic System of D.<sup>r</sup> Gall, of Vienna.*





*Instrument for extracting hard  
substances lodged in the Throat.*





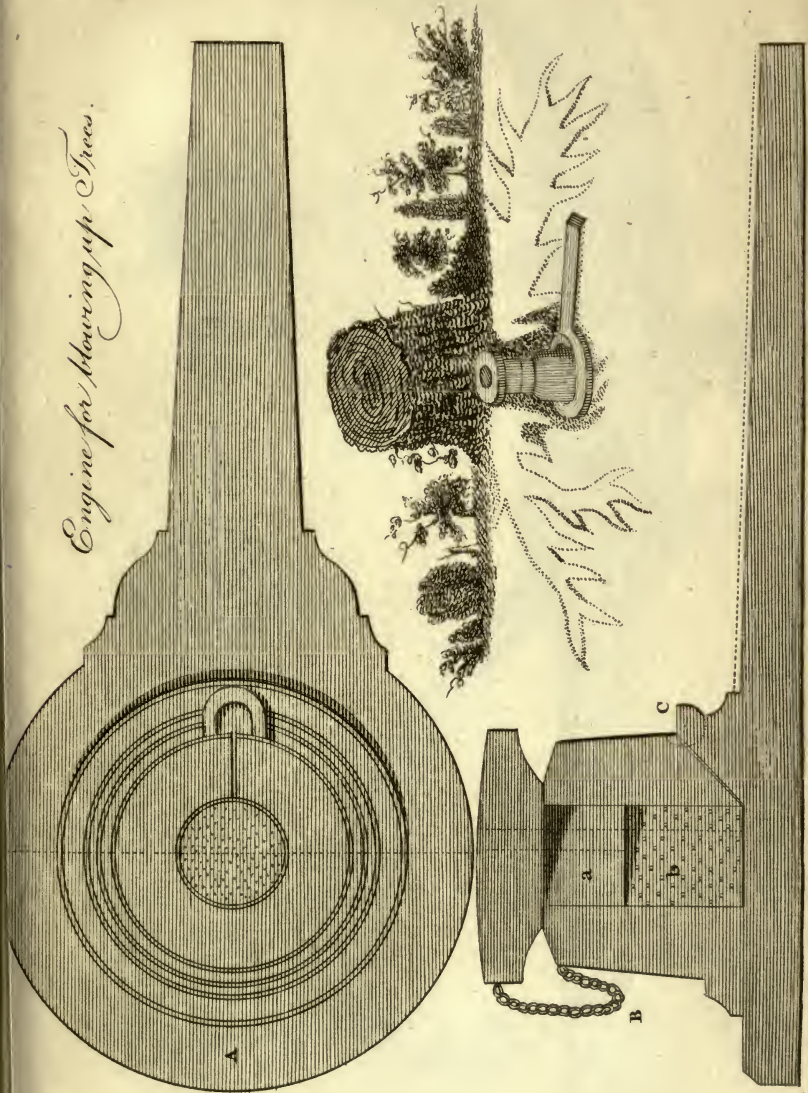








*Engine for blowing up Trees.*





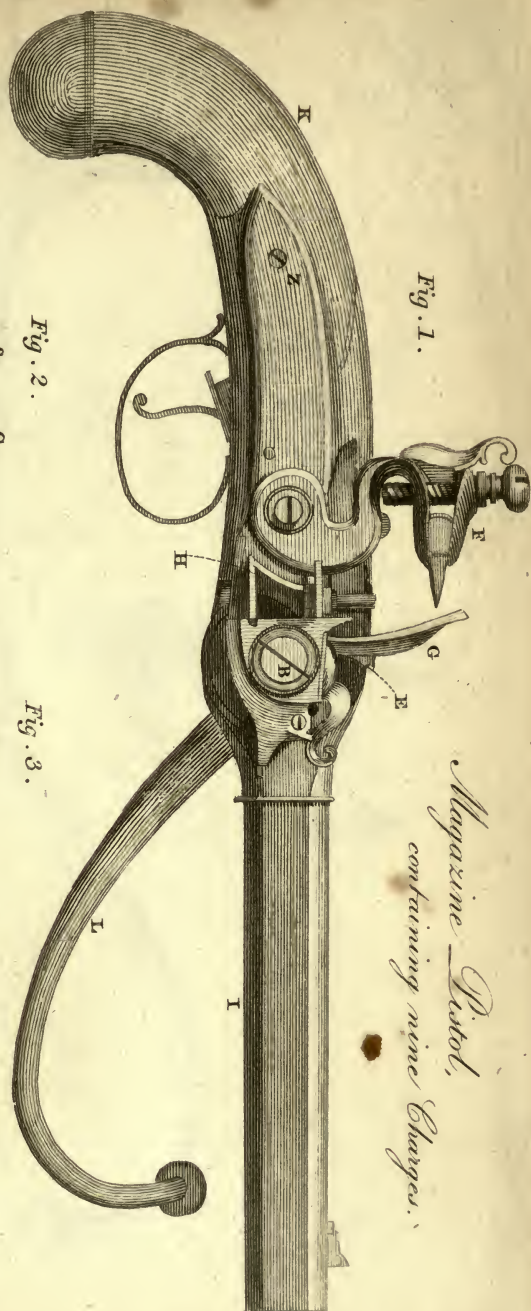


Fig. 1.

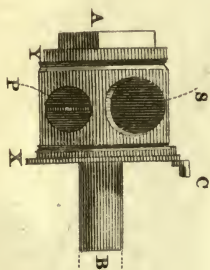


Fig. 2.

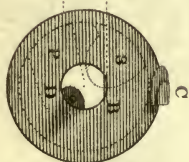
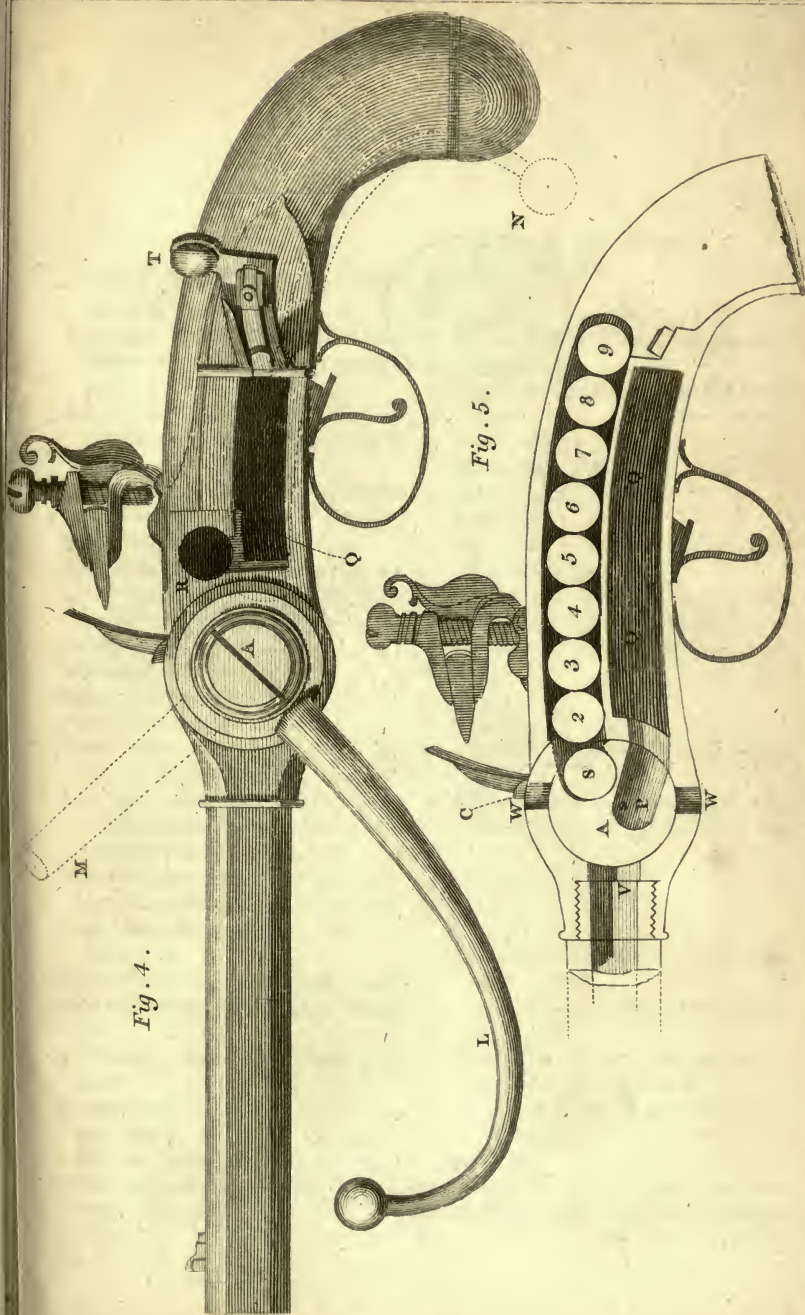


Fig. 3.

*Magazine - Triot,  
containing nine Charges.*











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